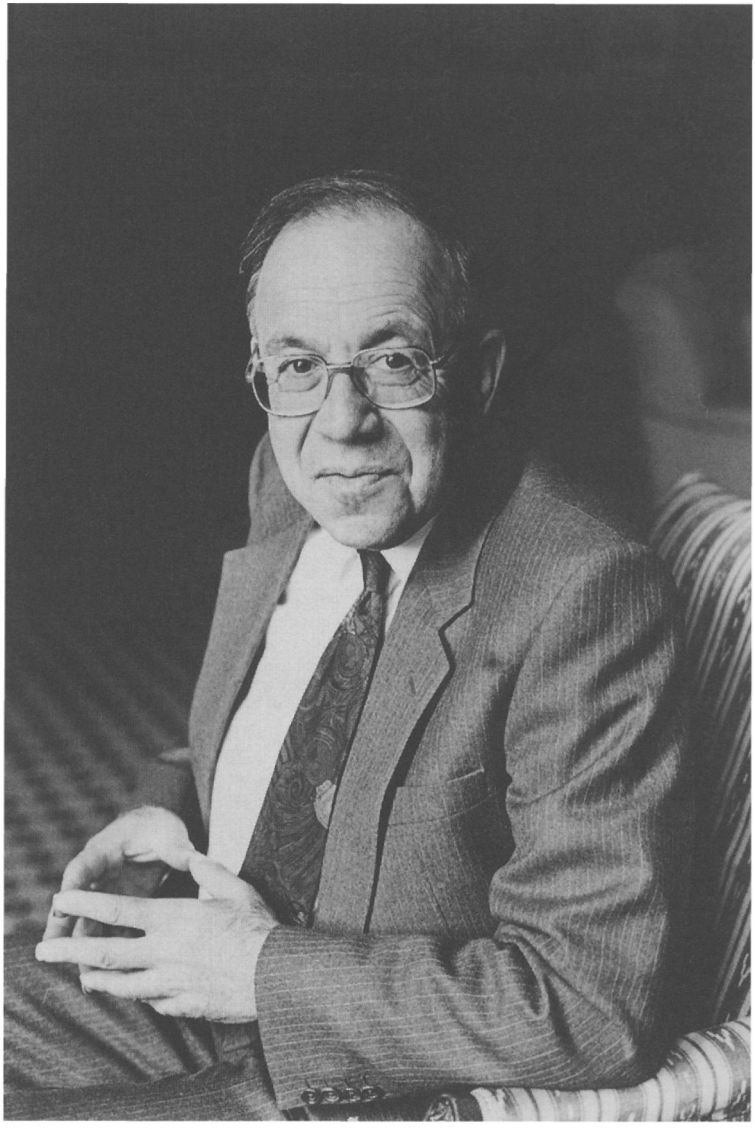
SELECTED PAPERS IN GREEK AND NEAR EASTERN HISTORY

DAVID M. LEWIS

David M. Lewis (1928–94) was one of the foremost historians of the ancient world, and was uniquely expert in both Greek and Near Eastern history. His name appears on the spine of numerous important books, but much of his most original and influential work was published in article form. The papers selected for this volume (four of them previously unpublished) illustrate the range and quality of his work on Greek and Near Eastern history and his particular expertise in dealing with inscriptions, ostraka and coins. Professor Lewis began considering the choice of papers for inclusion before his death and they have been prepared for publication by Professor P. J. Rhodes. A complete bibliography of the author's published works concludes the volume.

Professor Lewis's interests ran across the frontiers of many disciplines, and students of ancient Greek religion and literature, as well as historians, epigraphists and orientalists, will find many insights in this material.

Selected papers in Greek and Near Eastern history



David M. Lewis
(Photograph: Jane Brown)

Selected papers in Greek and Near Eastern history

DAVID M. LEWIS

Formerly Professor of Ancient History in the University of Oxford

Edited by

P. J. RHODES

Professor of Ancient History in the University of Durham

CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge, United Kingdom

CAMBRIDGE UNIVERSITY PRESS
The Edinburgh Building, Cambridge CB2 2RU, UK
40 West 20th Street, New York NY 10011–4211, USA
477 Williamstown Road, Port Melbourne, VIC 3207, Australia
Ruiz de Alarcón 13, 28014 Madrid, Spain
Dock House, The Waterfront, Cape Town 8001, South Africa

http://www.cambridge.org

© Cambridge University Press 1997

This book is in copyright. Subject to statutory exception
and to the provisions of relevant collective licensing agreements,
no reproduction of any part may take place without
the written permission of Cambridge University Press.

First published 1997
First paperback edition 2002

Typeface Adobe Caslon.

A catalogue record for this book is available from the British Library

Library of Congress Cataloguing in Publication data
Lewis, David M. (David Malcolm)
Selected papers in Greek and Near Eastern History / David M. Lewis;
edited by P. J. Rhodes.
p. cm.
Includes bibliographical references.
ISBN 0 521 46564 8
1. Greece – Civilization – To 146 BC. 2. Middle East –
Civilization. 3. Civilization, Ancient. I. Rhodes, P. J. (Peter
John) II. Title.
DF78.L46 1997
938–dc21 96-399750 CIP

ISBN 0 521 46564 8 hardback
ISBN 0 521 52211 0 paperback

CONTENTS

List of plates *ix*
Preface *xi*
Systems of reference *xii*

GENERAL

1 Boeckh, *Staatshaushaltung der Athener*, 1817–1967 *1*
2 On the new text of Teos *7*
3 The origins of the First Peloponnesian War *9*
4 The federal constitution of Keos *22*
5 The Athens Peace of 371 *29*
6 Preliminary notes on the Locri archive *32*
7 Temple inventories in ancient Greece *40*
8 Democratic institutions and their diffusion *51*

ATHENIAN

9 Public property in the city *60*
10 Cleisthenes and Attica *77*
11 Review of J. S. Traill, *The Political Organization of Attica* *99*
12 Review of P. Siewert, *Die Trittyen Attikas und die Heeresreform des Kleisthenes* *102*
13 The Kerameikos ostraka *110*
14 Megakles and Eretria *114*
15 The Athenian Coinage Decree *116*
16 Athena's robe *131*
17 The treaties with Leontini and Rhegion *133*
18 Entrenchment-clauses in Attic decrees *136*
19 Apollo Delios *150*
20 After the profanation of the Mysteries *158*
21 Aristophanes and politics *173*
22 Who was Lysistrata? *187*
23 A note on *IG* i^2 114 [= i^3 105] *203*

24 The epigraphical evidence for the end of the Thirty 205
25 The financial offices of Eubulus and Lycurgus 212
26 The dating of Demosthenes' speeches 230
27 Law on the Lesser Panathenaia 252
28 The Athenian *Rationes Centesimarum* 263
29 The chronology of the Athenian New Style Coinage 294
30 Review of M. Thompson, *The New Style Silver Coinage of Athens* 321

NEAR EASTERN

31 The Persepolis Fortification Texts 325
32 The King's dinner 332
33 Datis the Mede 342
34 Persians in Herodotus 345
35 The Phoenician fleet in 411 362
36 Persian gold in Greek international relations 369
37 The first Greek Jew 380
38 Review of J. N. Sevenster, *Do You Know Greek?* 383

Bibliography 389
Publications of David M. Lewis 400

Indexes
1 Index of texts treated in detail 412
2 General index 414

PLATES

1 Apollo Delios: *IG* i³ 130, frag. b (Centre for the Study of Ancient Documents, Oxford) *151*

2 The epitaph of Myrrhine: *IG* i³ 1330 (Epigraphic Museum, Athens) *188*

3 The base of Demetrios' Lysimache: *IG* ii³ 3453 *192*

4 Law on the Lesser Panathenaia: *SEG* xviii 13 (American School of Classical Studies at Athens) *253*

PREFACE

The Cambridge University Press first suggested to David Lewis in 1987 that it might publish a book of his. He gladly agreed to the publication of a volume of his selected papers, and started to think, and to consult friends, about what ought to be included in it, but he did not come to a final decision on the contents. Shortly before his death, in 1994, he asked me as his literary executor to take the final decision (which I have done after further consultation) and to edit the book.

Chapters 5, 21, 25 and 26 are published here for the first time. They are the papers from amongst his unpublished works which Lewis wanted to be included in this volume. He did some work on them in the last year of his life, but he left it to me to do the final editing, and to write the footnotes (though he gave some indication of what he wanted, and in some of the notes which provide more than bare references I have been able to use his words). Copies of some other unpublished papers are being deposited in selected libraries: see p. 411.

The remaining chapters have been published before, and I am grateful to the editors and publishers concerned for permission to republish them here. I have standardised the form and have done some updating of the references; I have supplied a first footnote (cued by an asterisk rather than by a number) for each chapter, including the details of its first publication; my more substantial interventions elsewhere are enclosed in square brackets.

Obituary notices have been published elsewhere (see especially that by S. Hornblower, *PBA* '1996 Lectures and Memoirs'), and there is no need for another here, but I am glad to have the opportunity of paying this tribute to the memory of a scholar who made contributions of great distinction to the study of the Greek and the Near Eastern world, both through his own work and through his generous support for the work of many others. I thank his family and those of his friends whom I have consulted in the preparation of this book, the publishers and the printers for their careful work on it, and Mr. C. J. Joyce and Dr. J. T. Rhodes for help with the proofs.

Except in this preface, all occurrences of the first person singular refer not to me but to David Lewis.

Durham P. J. RHODES

SYSTEMS OF REFERENCE

Citations of ancient texts should cause no difficulty. Details of modern books (apart from standard editions of ancient texts, excavation reports, such standard works as *The Cambridge Ancient History*, and a few works which are cited only once and for which details are given where they are cited) are given in the Bibliography.
Notice:

GG *Griechische Geschichte*
HG *Histoire grecque* or *History of Greece*

Details of articles in periodicals are given where they are cited; periodicals are normally abbreviated as in *L'Année Philologique*, with the usual Anglophone divergences (*AJP* etc. for *AJPh* etc.), but notice:

AM	*Athenische Mitteilungen* (i.e. *Mitteilungen des Deutschen Archäologischen Instituts, Athenische Abteilung*)
BSA	*Annual of the British School at Athens*
Sb. Berlin etc.	*Sitzungsberichte* of the Academy at Berlin etc.

Some Near Eastern periodicals are abbreviated as follows:

BIFAO	*Bulletin de l'Institut Français d'Archéologie Orientale*
CDAFI	*Cahiers de la Délégation Archéologique Française en Iran*
EVO	*Egitto e Vicino Oriente*
IEJ	*Israel Exploration Journal*
JEOL	*Jaarbericht 'Ex Oriente Lux'*
JNES	*Journal of Near-Eastern Studies*
MelUSJ	*Mélanges de l'Université Saint-Joseph, Beyrouth*
Rec. Trav. Eg. Assyr.	*Recueil de Travaux relatifs à la Philologie et à l'Archéologie Égyptiennes et Assyriennes*
ZAssyr	*Zeitschrift für Assyriologie und Verwandte Gebiete*
ZDPV	*Zeitschrift des Deutschen Palästina-Vereins*

Note: To give help in following up references, the original pagination of articles is indicated at the top of each page and the original page divisions are marked in the course of the text by a double line (||).

I

Boeckh, Staatshaushaltung der Athener, *1817–1967*

A venerated teacher, in whom the best of the Berlin tradition is still alive, once said firmly to me that he supposed that the essentials of the things that interested me had changed very little since Boeckh. I would not now endorse this view, and this morning I am neither fighting a campaign to encourage more reading of Boeckh as a source of information, even in Fränkel's third edition, nor advocating the sort of piety which led Fränkel to reprint all Boeckh's errors with warning footnotes. However, I do think that there are reasons to commemorate the hundred and fiftieth anniversary of the *Staatshaushaltung*, particularly before an epigraphical congress.

We see in it first the first great example of *Alterthumswissenschaft*, an attempt to grasp and describe essential elements in the life of a people where there were no classical forerunners to define the scope of the subject. The general impulse to see ancient life as a whole certainly came to Boeckh from his teacher Wolf. His early works however do not make straight for this goal. In Göttingen, Schleiermacher had given him Platonic interests, and student poverty in Berlin made him, in a strange collocation, the tutor of the fifteen-year-old Meyerbeer, who wanted to learn Greek and Latin for the sake of musical theory.[1] Plato and musical theory produced an interest in Pythagoreanism, in itself and in Plato; and problems of authenticity, in Plato and the tragedians, also interested him in these early years. A nearer approach to universalism came as he started serious work on Pindar, though that also started from musical interests. At least by 1808,[2] he had formed his aim of writing *Hellen*, which would be the crown of his studies,

* Published in *Acta of the Fifth Epigraphic Congress, 1967* (1971), 35–9 (Basil Blackwell).

[1] (F. W.) M. Hoffmann, *August Boeckh: Lebensbeschreibung und Auswahl aus seinem wissenschaftlichen Briefwechsel* (Leipzig: Teubner, 1901), 11.

[2] The date from Thiersch's letter, *ibid.* 230, the definition, *ibid.* 35.

presenting the results of his investigations of the Greek people in as full a form as possible, and in 1809 he gave the first of those lectures which developed over fifty-six years into what we know as the *Encyclopädie und Methodologie der philologischen Wissenschaften*, in which he defined the aims and principles of philological study. These lectures seem to have changed over the course of time in formulation rather than in essentials. He eventually adopted from Reichardt a definition[3] which pleased him and which he would have always assented to: 'Die Alterthumswissenschaft ist weder eine Geschichte der Literatur, noch der Kunst, noch der Religion u.s.w. – solche Geschichten hat man schon ohne dieselbe – || sondern eine Geschichte des Volkslebens, das aus dem Ineinandersein und Zusammenwirken aller dieser Momente besteht.'

At least one friend was already warning him in 1808 that none of the data needed for *Hellen* had ever been collected,[4] and by 1815 the horizon had shrunk drastically. Serious work on *Hellen*, he wrote then,[5] had started in 1813, and he now realised that many, many years of *Vorbereitung* would be needed. He had begun with an investigation of Greek political conditions, found no satisfactory preliminary work had been done; all was in raw chaos. He therefore wanted to make clear to himself the different branches of political life and had got stuck on financial matters, without doubt the most obscure and where he found the least enlightenment available. In general terms this sounds reasonable enough, but other more specific reasons have been offered for his choice of subject. Sandys,[6] without giving evidence, gives Wolf's prolegomena to his *Leptines* as an inspiration, and there obviously is a relationship, but the dedication of the *Staatshaushaltung* is to Niebuhr, who had already similarly broken new ground in his Roman History, and a letter to Niebuhr[7] claims that the impulse to the book came from Niebuhr's companionship and observations that Niebuhr had made on Heeren's views on ancient trade. This particular debt is not acknowledged in the text, and I confess to suspecting some exaggeration here. There is some evidence[8] to suggest that Boeckh's brash enthusiasm had recently been irritating Niebuhr, and Boeckh may have thought tact in order. Since however this letter is the one which goes on to say[9] 'Die Akademie der Wissenschaften ist und bleibt eine Leiche, und selbst der

[3] *Encyclopädie*², 21. [4] Hoffmann, *August Boeckh* 230f.
[5] To von Reizenstein, *ibid.* 35. [6] *History of Classical Scholarship* iii, 98.
[7] Hoffmann, *August Boeckh* 209. [8] *Ibid.* 78f. [9] *Ibid.* 211.

Magnetismus wird sie nicht auferwecken', a remark which I do not find in Harnack's history of the Academy, I am probably wrong to think that Boeckh was unsure of his ground with Niebuhr, and we should give his acknowledgement of debt full weight.

Boeckh created as much of a public stir by discussing what had previously not been discussed as Niebuhr had done by discarding large quantities of evidence which had been previously thought reliable. 'Als dies Werk zuerst erschien, war alles so neu, ging so weit über alles hinaus, was für irgendein Volk, irgendeine Zeit versucht war, dass die Bedeutung nur von ganz wenigen Philologen, vielleicht mehr ausserhalb der zünftigen Kreise gewürdigt ward.'[10] In England, for example, the book could be translated by G. C. Lewis,[11] who held a utilitarian view that the assembly of facts was an essential preliminary to a really scientific treatment of politics and morals,[12] with a firm preface to point out that Boeckh's economic ideas did not go beyond those to be expected of an educated Athenian of the age of Aristotle and that this had led him into several serious errors, but that the book was very useful all the same. Boeckh, who was sparing in the connections he made between ancient || and modern life and held a fairly straightforward view about the mere contemplation and understanding of Greek life being educational, seems to have been left unmoved by criticism of this kind, and the second edition continued, for example, to convert drachmae conscientiously into thalers and gröschen despite Lewis's protests at the uselessness of the process.

As far as philologists were concerned, Boeckh's friends and pupils were moved to veneration, but, as Wilamowitz implies, there was less reaction elsewhere. It is clear this demonstration of what *Alterthumswissenschaft* could do was meant to be read in Leipzig. The reference in the preface to modern philologists who confined themselves to *Sprachforschung* and had reduced themselves to *Silben- und Buchstabenkritik* is a clear reference to Hermann from a fortunate Boeckh who had not yet discovered that wide issues may sometimes turn on *Buchstabenkritik*, and a letter[13] shows that a

[10] Wilamowitz, *Geschichte der Philologie* 54.

[11] Lewis of course found Niebuhr's attitude to facts very peculiar; see Momigliano, *Contributo alla storia degli studi classici* 249–62.

[12] Lewis to Grote, *Letters of the Right Hon. Sir George Cornewall Lewis, Bart to various friends* (London: Longmans, 1870), 159.

[13] Hoffmann, *August Boeckh* 234.

tour de force on Digamma at the end of volume II was meant for Hermann. 'Ob freilich Hermann sich dadurch überzeugen lassen wird, ist eine andere Frage, denn er versteinert sich sichtbar. Ich wünsche, dass es uns nicht ebenso gehen möge.' From the fact that errors in the text of the first Kallias Decree which Hermann pounced on nine years later were already present in the treatment of 1817, I deduce that Hermann at this stage did not bother much with the *Staatshaushaltung*.

Mention of the first Kallias Decree leads to my second main point, the use of inscriptions in the book. Though Letronne was shortly to do something similar, I think it reasonable to claim the *Staatshaushaltung* as the first book on a Greek subject which used inscriptions freely not as curiosities but as integral parts of the evidence. The use is natural, the approach is modern, their evidence is not much forced. Of course, as yet, they are not helping all that much. In 1817, for instance, one small fragment of the assessment of 425 was all that was known of the tribute-lists. But even in the main text inscriptions take their place as equals beside the orators and the lexicographers.[14]

This use as evidence is of course far from the only part inscriptions play in the book. More than a quarter of the first edition is taken up by the twenty-two *Beilagen* on various inscriptions. This is not the antiquarian operation that the publication of Greek inscriptions had previously been. The principles of selection are fairly rigorous, and Boeckh strives to keep the texts relevant to the book. Even the two closing non-Attic texts are in themselves not alien to *Finanzwesen*, though the commentary on the Orchomenian text spends most of its time in territory where one feels that the editor of *CIG* has taken over from the author of the *Staatshaushaltung* and is giving a prospectus of the riches which lie ahead. The *Beilagen* should have achieved their purpose in showing that inscriptions were really worthwhile. ||

It is hard to evaluate the quality of epigraphic work done in 1817 and to forget that one now knows the answers or some of them. I would say that Boeckh did pretty well with these texts, if one excludes his basic tendency to treat the copy or copies as manuscripts. Given that tendency, many of his

[14] I am regretfully inclined to say that they take their place rather above the historians. The Thucydidean revival had not yet really begun, but this hardly excuses Boeckh for having discussed the attendance at the Athenian assembly without noting Thuc. VIII.72, an omission which he did note in time for the addenda, or for concluding firmly that the charges of embezzlement brought against Pericles were justified in a passage which had to wait until the second edition for a citation of Thuc. II.65 and a retraction.

corrections are good, even brilliant, and relatively few are very unfortunate. As he was to show in the debate with Hermann, he was prepared to suspend judgement and view without surprise the kind of Greek which might be put on stone. On institutions quite a lot went wrong for lack of evidence; no one could have deduced the existence of the conciliar year from the accounts he possessed. What I find really impressive is his general grip on the nature of the document which he is describing and how it fits into the general picture; this is sometimes very good. Speculation is well controlled, much better than in *CIG*, with only one serious exception.

Lastly we must consider the relationship of the *Staatshaushaltung* to *CIG*. By his own account, Boeckh had been working on the book since 1813. He found himself involved with inscriptions throughout; they were not only useful, they were indispensable. When he came to the Academy in 1814, it was natural that inscriptions should come to the top in a discussion of what the revitalised Academy should do for a liberated Prussia and a liberated Europe, and the drafts of proposal for the Corpus which were circulated in the spring of 1815[15] clearly owe much to the direction his interests had taken.

> Kein Zweig der Altherthumskunde bedarf nicht ihrer Hülfe: die Inschriften enthalten wichtige Urkunden für die innere und äussere Geschichte der Staaten, ihre inneren Einrichtungen, Gesetze jeder Art, Privatverhältnisse und dergleichen, welche nur aus ihnen mit der möglichsten Vollständigkeit erkannt werden können. Ein Theil der Paläographie beruht auf ihnen; selbst für die Geschichte der Sprache sind sie von äussersten Wichtigkeit. Aber da sie in wenigen Händen sind, ist das Studium der Inschriften gegenwärtig von den Philologen fast gänzlich vernachlässigt, und die Inscriptionenlehre erscheint den meisten wie eine geheime Wissenschaft. Es ist einleuchtend, dass durch dieses Unternehmen . . . dieses Studium wieder ein Gemeingut der Philologen werden wird.

The path which has led from *Hellen* to the Corpus is clear to trace. Boeckh is still engaged in *Vorbereitung*. A Corpus would, after all, take about four years with plenty of help from colleagues.

We do not of course hear of *Hellen* again, and neither the Corpus itself nor its effect on Boeckh's later life fall within my subject. Momigliano[16] has

[15] (C. G.) A. von Harnack, *Geschichte der Königlichen Preussischen Akademie der Wissenschaften* (Berlin: Reichsdruckerei, 1900), ii 375 n. 1.

[16] *Contributo* 180f., 384ff.

drawn our attention to his indifference to the ideas of his pupil Droysen and to the general lack of philosophical curiosity in his later correspondence, but no one would be rash enough to distinguish the relative parts played by age and inscriptions in the *Versteinerung* which he had seen in Hermann and feared for himself. I should however stress one rather surprising respect in which his convictions remained firm. However much attention he paid to inscriptions, || the founder of Greek Epigraphy continued to hold emphatically that their study was a means to an end. The preface to *CIG* i[17] criticises Wolf for having made *epigraphice* the twenty-third of the twenty-four philological arts. Boeckh denies that it is an art or a discipline at all, since its subject-matter is not uniform. 'Neque ullum in sese (habet), sed aliarum (servit) disciplinarum finibus.' Posthumously, in the *Encyclopädie*, the line is much the same.[18] Epigraphy has no topic peculiar to it; it is impossible to define. 'Sie ist daher keine Disciplin, sondern ein Aggregat von Kenntnissen.' I do not find the attempt he makes here to include epigraphy under *Literaturgeschichte* at all helpful or convincing, but I am sure that it is good for us to be made to wonder from time to time whether epigraphy exists. Presumably it does, or we would not be here this week. As it falls to me to be speaking particularly early in the week, I will, if I may, extract two thoughts from what I have been saying. Firstly, it should be our aim not to be *eine geheime Wissenschaft*, and, secondly, we should all have our *Hellen*, individual or communal, that is, we should know why we came to be looking at inscriptions in the first place.

[17] p. vii. [18] §102.

2

On the new text of Teos

In arriving at the same solution for Herrmann's a 21–2 as that of Gschnitzer reported by Professor Merkelbach above (*ZPE* 46 (1982), 212), I formed a clear preference for δ̣ι̣[ακοσ]ί[ο]ι̣σιν̣ over [ἑξ|ακοσ]ί[ο]ι̣σιν̣ in a 16–17. Both will fit the *stoichedon* pattern, but the photograph seems to show in a 16 the base and part of the right diagonal of delta in the penultimate space and the bottom of a vertical in the last space.

That the quorum should be 200 in Teos and 500 in Abdera will be some guide to their relative sizes at this point. Mathematically, it produces an engaging coincidence, since the proportions of their normal tribute to the Delian League, 6 and 15 talents, are exactly the same.

The closest parallels for the use of σὺν in this text are Delphic (see Roux, *L'Amphictionie, Delphes et le temple d'Apollon* 65–71; cf. L. Lerat, *RP* 3ᵉ série 17 (1943), 70–9). Closest of all to our text is *CID* i 13.32–7: τά[δ]ε ἔδοξ|-[ε]ν Δ[ε]λφοῖς πάτρι|[α] ἔμ[ε]ν τοῖς Σκιαθ|[ί]οι[ς] σὺν̣ τετρακα|[τί]α[ι] ψήφωι καὶ πλ|[έο]ν.

I need not join the argument as to whether, at Delphi, '400 and more' indicates the figure which is bound to be a majority of the citizen body. It is clear to me that, in our text, 200 at Teos and 500 at Abdera are intended to be the quorum of the court or of the assembly acting judicially which will be required for the imposition of these penalties. Cf., at Chios, *DGE* 688. 21–5: κἀγδικασάν|των τριακοσ|ίων μὴ 'λάσσο|νες ἀνηρίθε|υτοι ἐόντες, for a property dispute. At Thasos, the quorum for βίαια seems to have been 300 from an early period. See *Recherches sur l'histoire et les cultes de Thasos* i.37 no. 7. 7–8 ἀπενγυ||άτω ὁ [κατειπὼν τὴ|ν ἀπεγγύην] παρὰ τριηκοσίοισιν κατάπερ τῶν βιαίων, and the number recurs *ibid.* p. 139 no. 18 (ML 83), lines 3, 11–12 τριηκόσιοι κρινόντων δίκην δικάσαντες. On these uses of τριηκόσιοι

* Published in *ZPE* 47 (1982), 71–2. The text under discussion was published by P. Herrmann, *Chiron* 11 (1981), 1–30: cf. *SEG* xxxi 985.

without the article, I have a distinct preference for the quorum interpretation given by F. Chamoux, *REG* 72 (1959), 351–6, over Pouilloux's own view that an oligarchic council is referred to. For other numerical references to a quorum, see, e.g., *CID* i 9. B 9–10; *Inschriften von Erythrai und Klazomenai* 2. A 20–5 (not quite clear, despite recent unanimity); *IC* iv 162.1–2, 181.7.

It is the mark of an oligarchic constitution that few people can impose sentences of death and exile (Arist. *Pol.* IV. 1294B33). I have not yet found the converse asserted, but it will certainly hold, and the insistence on a quorum here adds body to Herrmann's view (p. 24) that our document has strong democratic leanings.

3

The origins of the First Peloponnesian War

It is a pleasure to contribute to a volume in honour of Malcolm McGregor and to be able to repay in some degree the debts I have contracted over the years to his helpfulness and co-operation, never more strongly manifested than at the time I write. He has always liked the central topic, however apparently well explored, and I hope that he may find something here to interest him in an inevitably simplified investigation of a very central topic indeed, the first major conflict between Athens and Peloponnesian states.[1]

That a topic is central does not mean that it is well studied. About twenty years ago, the Oxford examiners put the question: 'Examine Athenian strategy in the First Peloponnesian War.' The result was disastrous. As they reported ruefully later, 'Of the 52 candidates who answered this question, 38 answered it as if it referred to the Archidamian War. We concluded that we could not fairly penalise this mistake.' The candidates were not indeed to be blamed. Their tutors had been anxious to get on to *the* Peloponnesian War, and most of the text-books give up on the First Peloponnesian War, confining themselves to a bald paraphrase of the military operations recorded by Thucydides, without troubling to think much about the implications of these operations. There has been a good deal more solid thinking since 1959, but there may still be more to be said.

Most of the trouble, of course, arises from the fact that our connected tradition rests on only seven pages of Thucydides, pages chiefly composed of a plain narrative of facts, sparing in ascriptions of motive and in general explanation. To my mind, these pages, like the whole of the excursus of which they form part, rank little higher than brief notes; Thucydides may

* Published in *Classical Contributions . . . M. F. McGregor* (1981) 71–8 (J. J. Augustin Inc.).

[1] My thanks are due to Professor A. Andrewes and Mr G. E. M. de Ste. Croix, who read an earlier version, and to Miss L. H. Jeffery for last-minute help.

well have wanted to do a good deal more work on them. Fortunately, at the end of the excursus, he is rather clearer about what he has shown, what he regards as the essence of the period.

> All these things which the Greeks did against each other and against the barbarian happened in roughly fifty years between the retreat of Xerxes and the beginning of this war, years in which the Athenians made their empire firmer and themselves attained great power, and the Spartans, seeing this, did not try to prevent them except for short periods, and kept quiet most of the time, being even before this not quick at going to wars unless they were forced, and in part also prevented by wars of their own, until the power of the Athenians was clearly at its height and they were laying hands on their own alliance. (1.118.2)

Two things here are perhaps unexpected. The first need not detain us long. It is of course a fact that, if you wish to select a year for which to draw a map of the Athenian Empire at its greatest extent, and colour as much of the map Athenian colour as you can, you will pick some year during the First Peloponnesian War and not 431, just as you pick the death of Trajan for doing the same thing for the Roman Empire. Both operations are equally delusory. They do not represent Athenian or Roman power at their firmest, and I do not think that there is any widespread desire to || challenge Thucydides' judgement that in all essential respects Athenian power was at its height in 431.

The second point is more interesting. I have been totally unable to determine who it was who invented the phrase 'The First Peloponnesian War'. It is not ancient, and I do not think it goes far back into the last century. The trouble is that, once established, it may have played a part in creating an expectation that the actual war was a prefiguration of *the* Peloponnesian War and that Sparta was an important part of it. It is certainly true that a Spartan army was involved in what was probably the major battle of the war and that a Spartan army took the field and, without fighting, probably exercised a decisive influence in the last campaign. It is undisputed, I think, that Athens and Sparta were formally in a state of war after the Battle of Tanagra, with a suspension for the Five Years' Truce of 451. But it seems to me that there has been a dangerous progression from chapter headings like: 'Der erste peloponnesisch-attische Krieg und die ägyptische Expedition' (Busolt); and: 'Der Ausgang der Perserkriege und der erste Krieg Athens gegen die Peloponnesier' (Meyer) to those which prejudge the question:

'Der Konflikt der Großmächte' (Beloch); 'Athènes contre Sparte (464–454)' (Cavaignac); and 'Athens at war with Persia and Sparta' (Hammond). These last seem to me, on the evidence of the passage I have quoted, to be in conflict with Thucydides' view, which is certainly quite incompatible with any proposition like: 'From 461 to 446 it was a main object of Athenian policy to attack Sparta and of Spartan policy to attack Athens.'[2] To my mind, Thucydides was quite right; the First Peloponnesian War was not in any important sense a war between Athens and Sparta at all. Fortunately, a recent paper by Holladay[3] has carefully surveyed the evidence for Spartan participation, and I can concentrate here on the preliminaries.

I begin with a simplified account of my attitude to some of the essential elements in Peloponnesian affairs during the thirty years or so before our war starts. With respect to Sparta, the first great period of the Peloponnesian League ends with an episode of weakness around 500. Sparta's position is notably re-established by her great victory at Sepeia in, say, 494, which knocked out Argos as a great power for a generation and gave Sparta the prestige she needed for her contribution to the saving of Greece in 480–479. In the period immediately after the defeat of Persia, there is evidence for disagreement in the highest levels at Sparta as to the desirability of pursuing an aggressive foreign policy outside the Peloponnese. There is also good evidence of increasing or revived difficulty with her allies in Elis and Arcadia. These troubles are persistent, give rise to at least two major battles, and continue at least until, if they do not overlap with, the major threat to the Spartan position caused by the revolt of her Helot and Messenian subjects after what may have been a very destructive earthquake in the winter of 465/4. This revolt was extremely serious. How long it must be reckoned with as a factor in Spartan policy is not certain. The treatment of the problem in *The Athenian Tribute Lists*[4] is still the most persuasive of those which bring the revolt to an end in 461 or 460 by emending Thucydides' text, but, despite

[2] But see the important remarks by de Ste. Croix, *The Origins of the Peloponnesian War* (henceforth *OPW*) 50–1, 180, for whom Thucydides is certainly wrong. He attributes Thucydides' failure to include the First War in his great design to his lack of information and to the fact that Sparta's allies did most of the fighting. For him, the lack of Spartan participation is due to military considerations, not to lack of will.

[3] *JHS* 97 (1977), 54–63. What Holladay has to say in 61 note 40 and 63 runs close to my own guesses about the importance of the Persian War for Spartan attitudes in my *Sparta and Persia* 63.

[4] iii. 162–8.

my earlier adhesion to that view,[5] I am now inclined to think that it went on longer than that and was still going, though perhaps in a rather minor way, until 456. If so, it will certainly have overlapped the opening phases of our war and have been an element in it.

Into this Spartan history, there interlocks from time to time the history of Argos, and here all is nearly dark. The battle of Sepeia was followed by some change of regime at Argos. After an || unknown length of time, there was counter-revolution, the new regime was thrown out and its members retired to Tiryns. At some time in the middle of the 460s Argos fought and captured Tiryns and Mycenae. The regime at Argos, probably the new regime rather than the counter-revolution, though this is by no means securely established, certainly co-operated to some extent with the anti-Spartan movement in Elis and Arcadia. The most thorough study, that by Forrest,[6] does not allow the counter-revolution a very long life, but I have no great confidence in this view.

These matters have been much explored. My interest here is in opening up newer ground in the exploration of the policy of the second city of the Peloponnesian League, Corinth, the great Greek city of whose inner life we know far the least.[7] To go back a little again, the historical evidence is clear in showing that, for thirty years or so, from 518 to about 488, it was an important part of Corinthian policy to nourish the growth of Athenian power,[8] and there is no doubt that the main reason for this was to set Athens against Aegina, at that time the dominant naval power in the Saronic Gulf. The symbol of this attitude is the remarkable act of lend-lease by which, perhaps in the year of Marathon, Corinth sold Athens 20 ships for use against Aegina at 5 drachmae apiece.[9] The political contrast with the period of the

[5] *Historia* 2 (1953/4), 412–18, but see *Sparta and Persia* 46, and *CAH*, v². 500.

[6] *CQ* n.s. 10 (1960), 221–32. My general difficulty with this treatment is a doubt about the way in which the sons of the slain allow their class-feelings to lead them to the side of the Spartans who killed their fathers; my particular problem is the need to make them lose control again after their coup in time for their opponents to make the alliance with Athens. R. A. Tomlinson, *Argos and the Argolid* 107 sees a more balanced situation at Argos, with the scales tipping from time to time.

[7] The only useful treatment I know of this period which puts Corinth in the centre of the picture is W. Grüner, *Korinths Verfassung und Geschichte*, intelligent but incomplete.

[8] de Ste. Croix, *OPW* 211.

[9] I deliberately keep trade out of the text. Corinthian friendship for Athens in this period is after all the most powerful warning that one cannot write political history

First Peloponnesian War is marked. At the beginning of that war Corinth is straining every resource to support Aegina against Athens. Her change of front seems to be due to a simple calculation of the balance of power; Athenian power, which increased greatly thanks to the shipbuilding programme before Xerxes' invasion and the success during it, now far outweighs Aegina's.[10]

That Corinthian policy in this period has its self-assertive elements has been seen most clearly in her relations with north-western Greece and in her attempts to strengthen her control over her colonial dependencies, attempts which start, with the case of Leukas, in the 470s.[11] For matters which concern us directly, we must start with an anecdote in Plutarch's life of Cimon, which has long been seen to rest on an eyewitness account by Ion of Chios.[12] Cimon was marching back from Sparta[13] and took his army through Corinth. A Corinthian named Lachartos complained to Cimon that he had brought in his army without asking the city's permission.[14] He said that it was customary to knock at other people's doors and not come in until invited by the master of the || house. To this Cimon replied: 'But you Corinthians, Lachartos, did not knock at the doors of the Cleonaeans and Megarians, but took a hatchet to them and forced your way in under arms, thinking that all doors were open to those who had the greater power.'

With Megara, we are on comparatively firm ground. Border troubles

from pots, since it is not all that long since Attic pottery had driven Corinthian off the world market. Corinthian potters (not necessarily her traders) had gone bankrupt, but the government did not care. However, it does not follow that Corinth's attitude to Aegina had no element of commercial consideration about it, since we cannot be sure that Aegina did not use her naval strength to promote her own entrepot trade. That Corinth's interests lay more in the West than in the Aegean (de Ste Croix, *OPW* 212) is something that we can hardly know, once the pottery index fails us.

[10] For de Ste. Croix, there is no hostility between Athens and Corinth before the Athenian alliance with Megara. He is right to match Plut. *Them.* 24.1 (Corinth accepts Themistocles' arbitration over Leukas) against Herodotean gossip about 480, but, if Themistocles' decision in favour of Corcyra made him a *euergetes* there, Corinth may at least have been mildly displeased, and I think de Ste. Croix undervalues Corinthian determination to preserve Aegina.

[11] A. J. Graham, *Colony and Mother City in Ancient Greece* 128ff.

[12] Plut. *Cim.* 17. 1–2; see most recently F. Jacoby, *Abhandlungen zur griechischen Geschichtschreibung* 156–7 (= *CQ* 41 (1947), 9).

[13] There is no need to consider whether it was from Ion that Plutarch derived his two expeditions of Cimon to Sparta.

[14] Cf. Thuc. IV.78.2 and Gomme's puzzled note; perhaps cf. Thuc. IV.75.2.

between Corinth and Megara were traditional, and Thucydides (1.103.4) tells us that the reason why Megara left Sparta and joined the Athenian alliance was that the Corinthians were making war on them because of frontier disputes. There is thus no doubt that one aspect of Corinthian policy between 480 and 460 was an aggressive policy on her northern frontier against Megara, though we cannot tell how early it developed. It should be noticed that Corinth is here acting with hostility against a fellow-member of the Peloponnesian League, with no apparent thought of, or reaction from, a much-preoccupied Sparta.

Cleonae is something different (with no confirmation from Thucydides). It lies between Corinthian and Argive territory. It has a plain of its own, but communications are not particularly difficult either with Corinth or Argos.[15] It falls slightly more logically into the Argolid, but it is as far from Argos as it is from Corinth, and, in a period of Argive weakness like that which followed Sepeia, it would be vulnerable to Corinthian aggression. Cimon's remarks assure us that there had been such aggression. Why would Corinth be interested in Cleonae? At all relevant periods, the only interesting thing about Cleonae is its control of the Nemean Games,[16] a control which it normally exercises under the suzerainty of Argos. The introduction to the Scholia on Pindar's Nemean Odes, however, says that Corinth did once control the Nemean Games.[17] It gives no date, and perhaps we have no right to use the information here. However, we are certainly entitled to believe

[15] Tomlinson (note 6 above), 29–30.

[16] Pind., *Nem.* 10.42, 4.17, *Hyp, Schol.* Pind. *Nem.* (*Scholia vetera in Pindari carmina*, ed. Drachmann, iii, pp. 3.17, 5.3).

[17] This is the position established by McGregor, *TAPA* 72 (1941), 277–8, and he was surely right. If I correctly understand S. G. Miller, *Hesperia* 46 (1977), 9–10, he thinks that there is no evidence even for Argive suzerainty before the end of the fifth century (G. W. Bond, *Euripides Hypsipyle* 144) and the beginning of the fourth (Xen. *Hell.* IV.7.2). Scepticism about Eusebius' attribution of the foundation of the games to the Argives goes back at least to Grote (*H.G.* iii. 481 n. 1 in the 12-volume edn, iii. 290 n. 3 in the 10-volume edn), and he also argued that the reference to the Cleonaeans in Pind. *Nem.* 10, which is an ode for an Argive, proved that there was no dispute over the games at the time between Argos and Cleonae and that Argive control therefore started after *circa* 460. It seems to me safer to distinguish between actual running of the games and suzerainty. That in D.S. XI.65.2 the Argives regard Mycenaean claims to the games as an affront seems to me to show that they already claimed such suzerainty. The physical transfer of the games to Argos which Miller's excavations at Nemea are currently suggesting is a different matter again; the abandonment of the site seems too late to concern us here. For the date of *Nem.* 10, see Forrest (note 6 above), 228.

that the control of the games was an issue in this period of Argive weakness. Diodorus (XI.65.2–3) gives us the reasons which led the Argives to attack Mycenae, probably in 465 or 464.[18] The Mycenaeans had been getting above themselves. They were disputing with Argos about the control of the Argive Heraion, and there is indeed some slight epigraphical confirmation that Argos did lose control of the Heraion in this period.[19] They claimed that they should run the Nemean Games themselves. Presumably this would be resented in Cleonae as well as in Argos, and some 'cross-check' on this is given by the statement in Strabo[20] that Cleonae joined Argos in her attack on Mycenae. Unfortunately the point is neutralised by the evidence of Pausanias that some of the defeated Mycenaeans moved to Cleonae. We may either reflect on the difficulties presented by the use of scattered, secondary sources or suppose that there were two sorts of Mycenaeans, which is likely enough.[21] ||

It seems most economical to associate these two pressures on Cleonae and the Nemean Games and to suggest that Mycenae was in some sense an instrument for Corinthian pretensions and aggressions in the northern Argolid. The attack on Cleonae is sufficient evidence for these aggressions, even if Mycenae was playing an entirely lone hand, which seems hard to believe. The evidence which is missing here, of course, is direct evidence of Corinthian support for Mycenae. It is tempting to supply some. Olympia has produced two helmets and parts of at least six shields with the inscription 'The Argives dedicated these to Zeus from their spoils from Corinth.'[22] The dedication has most recently been dated 500–475.[23] Kunze and Miss Jeffery, who once followed him, wisely did not offer an occasion for an Argive defeat of Corinth in those years, and I find it hard to think of one. One would certainly have to go back nearly to 500 before the darkness is thick

[18] For the date I follow Andrewes, *Phoenix* 6 (1952), 3 and Forrest (above, note 6), 231–2, who is less confident. Tomlinson (note 6 above), 104–8, inclines to prefer Diodorus' date of 468/7, but this only derives from Diodorus' method of laying out his material and is not evidence.

[19] Mitsos, Ἐπετηρὶς τῆς Ἑταιρείας Βυζαντινῶν Σπουδῶν, 21 (1953), 150–1, Forrest (above, note 6), 230.

[20] VIII.6.19, p. 377.

[21] Pausanias (VII.25.6) appears to contrast those who moved to Cleonae with the *demos*.

[22] References collected by L. H. Jeffery, *The Local Scripts of Archaic Greece* 162, 169 note 18.

[23] E. Kunze, *Bericht über die Ausgrabungen in Olympia* 5 (1956), 36, a slightly lower date, based on a changed view of the London helmet, than that implied in *Bericht* 3 (1938–9), 77. The dedication was removed by the middle of the century, which is a separate problem.

enough to bury these weapons.[24] I am relieved to be told by Miss Jeffery, at least, that she now sees no obstacle to placing this victory around 465.[25]

Does this exhaust the story of Corinthian expansion? I do not think it does, but, to take it further, we have to work back from the opening events of our war itself. The first action in the war was an Athenian landing at Halieis. This was met and defeated by a combined force of Corinthians and Epidaurians. Thucydides' list of Peloponnesians is not exhaustive; we now know that there were Sicyonians there too.[26] Close co-operation between Corinth and Epidaurus continues to be attested in the next phase, when everyone's attention switches to the battle for Aegina. In fact, the Corinthians and Epidaurians seem to be the principal Peloponnesians against whom the Athenians fought at the beginning of the war. We have the right to enquire about this close association between Corinth and Epidaurus. Marriage relations go back to the time of the Corinthian tyranny,[27] but Epidaurus has some sacred obligations to Argos[28] and would be vulnerable by land to her in a period of Argive power. We also have the right to ask why the Athenians were attacking Halieis and, more especially, why the Corinthians, Sicyonians and Epidaurians were there to defend it, the first two at least very far from home. Thucydides does not stop to tell us, and very few have stopped to ask.[29] The answer seems fairly obvious. Profiting again from

[24] Even if, with Jeffery, *Archaic Greece. The City-States c. 700–500 B.C.* 157–8, one accepts a late sixth-century war of Megara and Argos against Corinth, based on Paus. VI.19.12–14, who puts it in the tenth century, and the Megarian Treasury at Olympia, neither the weapons nor the lettering can be that old.

[25] So also Forrest (above, note 6), 231 note 2.

[26] As far as I know, the only published references to the greave (Olympia B 2777) found in the south wall of the Stadion in 1940 remain the newspapers Ἑστία and Ἐλεύθερος Κόσμος of 8 May 1971. I conflate published and unpublished texts since the published spellings are impossible: τοὶ Σεκυόνιοι ἀνέθεν τοῖ Δὶ ἐξ Ἀλιέο[ν] Ἀθεναίον *h*(ε)λόντες. For the same grouping at the time of the Megarian revolt of 446, cf. Thuc. I.114.1. For the development of a Corinthian *Sonderbündniß*, see Grüner (note 7 above), 28. But Sicyonian policies are not invariably aligned with Corinthian after the Thirty Years' Peace; they sent no ships to Leukimme and joined Sparta in attempting to solve the Corinth-Corcyra dispute (Thuc. I.27.2–28.1).

[27] Her. III.50.2. Even E. Will, *Korinthiaka* 544 sees Corinthian maritime interests in this marriage.

[28] Thuc. V.53, with W. S. Barrett, *Hermes* 82 (1954), 421–42.

[29] Some of the necessary points are made by Jeffery, *BSA* 60 (1965), 53–4, but she has her mind on the Sparta-Argos relationship and does little to explain Corinthian involvement.

the period of Argive weakness, Corinth has established fairly strong interest in the eastern Argolid or the area east of the Argolid, whichever way you like to put it. We are indeed at the start of the war, and past the high point of Corinth's power in the Argolid. Argos has recovered far enough to regain Mycenae and Tiryns, but outside the Argive plain itself Corinthian influence will remain strong, and the observation is confirmed by the fact that the Tirynthians expelled by Argos went either to || Epidaurus or Halieis.[30] The fact that Argos is now once more in the way by land is of course not particularly important to Corinth. Communications from Corinth to Epidaurus and beyond are by sea, and the importance to Corinth of this area lies in having firm control of one coast of the Saronic Gulf, her outlet to the Aegean. As she becomes more and more suspicious of Athens' growth, this becomes more important. The key to the Saronic Gulf is of course Aegina, and we have no means of telling when Corinth made her switch of sides between Athens and Aegina; all we do know is that by the start of our war she and Epidaurus are prepared to regard Aegina as vital. If we move on forty years, we find Thucydides (v.53) rather more ready to illuminate the strategic importance of this part of the world. Alcibiades has promoted a new alliance between Athens and Argos; Aegina is now an Athenian island. Alcibiades and the Argives agree that Epidaurus must be won, (*a*) to keep Corinth quiet, (*b*) because communications between the Athenians on Aegina and Argos would be quicker that way than sailing round Cape Skyllaion.[31] The fall of Epidaurus would mean both the completion of the Athenian ring across the Saronic Gulf and the assurance of communications between the Argive and the Athenian spheres.

We can now return to what is perhaps the major turning-point in the *pentekontaetia*, the Spartan decision to dismiss the Athenian force which had come to help them at Ithome. This finally sealed the failure of Cimon's version of Athenian foreign policy: pursuance of the war against Persia founded on friendship with Sparta. Athens had become too different for the partnership to be possible. Cimon fell and, by formal notice or not, Athens abandoned the Spartan alliance. The pattern of Greek friendships as the Persian invasion had left it was at an end. Within a few months, Athens was in alliance with Argos, neutral in that war and perpetually hostile to Sparta,

[30] Strabo, VIII.6.11 p. 373, following the version of W. Aly, *PP* 5 (1950), 244.

[31] I repress the temptation to discuss Thuc. II.56.4.

with Thessaly, pro-Persian in that war (where a Spartan king had met disgrace some fifteen years before), and with Megara, bitterly discontented with the aggression from Corinth which the absence of Spartan leadership had made possible. This, says Thucydides (1.103.4), was the principal reason why the Corinthians first began to hate the Athenians so bitterly.

What did the new partners want? Some recent treatments attribute their alliance, at least from the Athenian point of view, to fear of Sparta: to worry lest the hostility revealed by the dismissal of Cimon be rapidly transformed into Spartan military action.[32] I find this very hard to believe. On any view, Sparta's Messenian War is not yet over at the time of the alliance with Argos and Thessaly, and, since Athens embarks before long on the Cyprus expedition which turned into the Egyptian expedition,[33] it seems hard to think that full-scale war with the whole Peloponnesian League was envisaged or that Sparta was thought a major threat. I prefer the more normal view, that Athenian policy was expansionist and that the Spartan alliance would no longer be allowed to stand in the way of implementing policies which had been in many Athenian minds for many years. Athens' initial concern will have been to have her final, long-delayed settlement with Aegina. As for the others, I have nothing to add on Thessaly, about which one can never say anything without qualification.[34] Megara wanted security from Corinth and this she got; Athens || gave her long walls and a garrison. Argos wanted self-respect and at least the re-establishment of her position in her area. If my argument so far is followed, it is clear that the wishes of at least Athens, Argos and Megara brought them into conflict above all with Corinth[35] and

[32] Notably de Ste. Croix, *OPW* 182–3, but already Jeffery (note 29 above), 52; my impression is that this is a very new idea indeed. For de Ste. Croix, Athenian motives are entirely military: Argos to bring hoplites, Thessaly cavalry, Megara a landbarrier against invasion from the south. The only evidence which is produced to show that there might be military danger is the alleged Spartan promise to invade Attica at the time of the Thasian revolt (Thuc. 1.101.1–2). We do not know that this was the time when this came to be known or believed in Athens; whether it was true is uncertain, but hardly relevant.

[33] I accept de Ste. Croix's contrast between Athens' power by sea and her vulnerability by land, but no one will suppose that fewer than 2000 hoplites went to Cyprus and there may well have been many more; cf. Thuc. 1.105.3, where the *stratia* which is away in Egypt must in the context be hoplite.

[34] Some suggestions in Jeffery (note 29 above), 52 note 49.

[35] That the principal target of the allies was Corinth has been said most clearly by Tomlinson (note 6 above), 113, but I find it hard to believe that there was, at this

her partners Sicyon and Epidaurus. They had no immediate need to come into conflict with Sparta at all.[36] If Sparta was unwilling or unable to defend her allies,[37] she just did not come into question. In the first phase of the war, the campaigning season of 459,[38] she certainly did not. This first phase ends with a major Corinthian defeat outside Megara and the completion of the Athenian blockade of Aegina, of which the Aeginetan capitulation of the next year was only the consummation. Essentially, this first year's campaign had given the allies most of what they had been fighting for. Corinth, it seems to me, had been cured of meddling for some time. Athens had gained control of much of the Saronic Gulf. At some later stage, she improved her position by taking over Troizene[39] and Hermione[40] on the further shore. Halieis too was at some point for a time in hands not friendly to Sparta, though the Tirynthians were not disturbed.[41] Only at Epidaurus had the allies perhaps hoped for more than they achieved.[42]

As wars go on, they are more and more likely to form their own pattern and less likely to throw light on their causes. I have nothing new to add on Sparta's motives for the Tanagra campaign, though I have a preference for taking Thucydides (1.107.2) at face value and supposing that the only motive

stage of Corinthian history, any prospect or intention of turning Corinth into a friendly democracy.

[36] The thought has of course suggested itself that Argos ought at this time to have wanted to recover the Thyreatis from Sparta as in 421 (Thuc. v.41.2); Jeffery (note 29 above), 53, Tomlinson (note 6, above), 108–9, 111–12. But, as far as we know, no attempt of the kind was made, and the only possible conflict of Argos and Sparta in the Peloponnese at this time was on Argive territory at the battle of Oinoe, carefully discussed by Jeffery, but which may never have happened (Andrewes in *The Ancient Historian and his Materials . . . C. E. Stevens*, 9–16). Either we have lost something, which is by no means impossible, or the Athenians had made it clear to the Argives how far they were prepared to go.

[37] For de Ste. Croix, *OPW* 187–96, there was no lack of will, and he makes a stronger case for the difficulties in the way of Spartan intervention than Holladay (note 3 above, 61) perhaps allows, though I agree with Holladay that it is the will which needs demonstrating.

[38] *Pace ATL* iii. 177 note 60, but I agree that only one year is involved.

[39] Thuc. 1.115.1; IV.21.3. [40] *SEG* X.15 (= *IG* i³ 31).

[41] Her. VII.137.2, where a Spartan captures it.

[42] Miss Jeffery reminds me of an Epidaurian proxeny decree for an Argive which could well belong to the 450s (W. Peek, *Abh. Leipzig* 63.5 (1972), 9, no. 10), but there is no necessary political implication, even if we could be sure of the politics of Philoxenos son of Phylacidas. Epidaurus is certainly on the Corinthian side at the end of the war (Thuc. 1.114.1).

was Doris. I am certainly not denying that Sparta was in the war after a fashion thereafter or that Tolmides burnt the Spartans' dockyard at Gytheion. What I am inclined to say is that, once Athens had established her control in Boeotia,[43] her main objective continued to be Corinth and that most of the scattered operations which follow are directed at restricting Corinth's activities in the Corinthian Gulf in the same way as the opening phase had already done for the Saronic Gulf. Someone, perhaps Tolmides,[44] settles Messenian exiles at Naupactus. Tolmides seizes Chalkis from Corinth.[45] Pericles, a year or two later, takes over Achaea and makes an attack on Oiniadai,[46] with which the Messenians were also engaged.[47] ||

One topic demands slightly closer attention. Thucydides[48] gives both Tolmides and Pericles attacks on Sicyon and minor victories. One would be inclined to dismiss these as minor attacks on Corinth's ally of purely nuisance value, but Plutarch has a rather different story about Pericles: 'He was admired and his name renowned in the outside world for his campaign against the Peloponnese with a hundred triremes from Pegae in the Megarid. For he not only sacked much of the coast like Tolmides before him, but he even went a long way in from the sea with the hoplites from the ships, so that everyone else cowered behind their walls at his approach, but the Sicyonians fought him at Nemea, and he beat them soundly and set up a trophy.'[49] The orthodox treatment of this passage[50] is to say that Plutarch or his source has confused Nemea with the River Nemea which formed the boundary between Sicyon and Corinth, and that Pericles never went inland at all. I dare say this is right, but there is a perfectly passable route inland, and Pericles might have been doing something to protect Cleonae's rights to Nemea.[51]

[43] I allow myself a wicked and irrelevant guess on a problem which has recently been concerning our honorand. In *ATL* List 2, col. ix.9 [Κλαζομ]ένιοι used to be read, but they have had to be abandoned since their appearance in the new fragment of col. viii. McGregor, *ap.* Camp, *Hesperia* 43 (1974), 317 and in *Hesperia* 45 (1976), 281, abandons a clear iota and suggests [Κυζζικ]ενοί. Are we so certain that no tribute can have been paid to the Delian League by the epigraphically preferable [*h*ερχομ]ένιοι (cf. *SEG* x.84.24 = *IG* i³ 73.23 for the spelling)?

[44] D.S. xi.84.7. [45] Thuc. i.108.5. [46] Thuc. i.111.3.

[47] Paus. iv.25, cf. Jeffery (note 22 above), 205. That the independent position of Oiniadai reflects some Corinthian interest is only a guess; contrast the slightly different view of G. B. Grundy, *Thucydides and the History of His Age* 347–54.

[48] i.108.5, iii.2. [49] Plut. *Per.* 19.2.

[50] See, e.g. Busolt, *GG* iii. 1 334 note 4.

[51] Cleonae itself was of course under Argive control in the year of Tanagra (Paus. i.29.7) and apparently earlier (see notes 20–1). The start of the route, at modern Assos, seems

We are of course affected by the usual frustrations of seeing these events through the eyes of Athens and our inability to get inside Corinth. What I have been describing as Corinthian aggression might have been seen there as a quest for security, and the Corinthians will certainly have felt themselves encircled as the new alliance got going.[52] We might hazard the guess that the renewed interest in the colonies and the border-war with Megara point to some population pressure. We do have a picture of Corinth in this very period, Pindar's Thirteenth *Olympian* of late 464 or early 463, but it does not help us much.[53] Pindar does say:[54] 'Among you the Muse, sweet-spoken, among you Ares also, flowers in your young men's spears of terror', but that is a compliment, amply justified by the Corinthian record in the Persian War and more than neutralised by what has been said before about the Hours: 'Corinth the rich, forecourt of Poseidon of the Isthmus, shining in its young men. There Law, sure foundationstone of cities, dwells with Justice and Peace,[55] dispenser of wealth to man, her sisters, golden daughters of Themis, lady of high counsels. They will to drive afar Pride, the rough-spoken, mother of Surfeit.' I once wondered whether it was Athenian *hubris* which was in mind, and whether Pindar was already assured that, if it came to a showdown, Corinth would stand by her friends in Aegina. That would be a very unfashionable thought for the 1970s and is moreover wrong. Internal Koros and Hubris are things with which Eunomia is amply qualified to deal,[56] and we learn nothing from this poem about Corinth's foreign policy.

too close to Corinth for comfort. Whether the Athenians ever came near these parts or not, note the possible Argive victory at Phlius (Jeffery (note 29, above), 53 note 50, slightly more venturesome than *Local Scripts* (note 22 above), 146 note 1).

[52] Cf. E. Meyer, *Geschichte des Altertums* iv. 1 556.

[53] I have not yet thought of a way to exploit politically the brilliant expansion of the family background of the victor by W. S. Barrett in *Dionysiaca . . . Sir D. Page* 1–20. We now know more about the Oligaithidai than about any other classical Corinthian family.

[54] *Ol.* 13.22–3 in Richmond Lattimore (tr.), *The Odes of Pindar* 37.

[55] Wilamowitz, *Pindaros* 372, thought that Eirene was important because of the developing war between Sparta and Athens. This seems unnecessary and unlikely; that Eirene can be thought of for internal reasons is clear enough from West's commentary on Hesiod, *Theog.* 902. Wilamowitz (369 note 1) is surely likely to be right in thinking that the reference to Enyalios in line 106 is athletic. The concluding prayer for *aidos* (line 115) refers to the successes of the family, not to the city; cf. *Ol.* 7.89, precisely contemporary.

[56] Cf. Solon, 4.32–4 West.

4

The federal constitution of Keos

No fewer than three of the inscriptions in the second volume of Dr Tod's *Greek Historical Inscriptions* are directly concerned with Keos, and this encourages me to hope that he may find interest in this investigation. It arises from an inscription from Ioulis, published by Dunant and Thomopoulos,[1] which is a close parallel to *IG* xii. 5 594 (Tod 141), the sympolity-treaty between Keos and Hestiaia. It is nearly certainly a treaty with Eretria. It raises several interesting problems, but I should principally like to draw attention to the remarkable federal constitution of Keos which the two inscriptions taken together reveal.[2]

I repeat the new text for convenience with some slight alterations. It is *stoichedon*, uses o for ου, but the letter-forms can hardly fix it closer than 390–340.

[--- ἐὰν δὲ ὁ Κεῖος βόληται πολιτεύεσθα]-
[ι ἐν Ἐρετρί]ηι, ἀπ[ογ]ραψά[σθω τὸ ὄνομα τὸ αὑτ]-
[ὸ, οἱ δὲ στρ]ατηγοὶ φυλὴν κ[αὶ χῶρον δόντων α]-
[ὐτῶι ἐν] ᾧι ἂμ μέλληι πολιτεύε[σθαι· ἐὰν δὲ ὁ]
[Ἐρετρ]ιεὺς βόληται ἐγ Κέωι πολ[ιτεύεσθαι]
[ἀπογ]ραψάσθω πρὸς τὸς θεσμοφύλακ[ας τὸ αὐ]-
[τὸ ὄ]νομα, οἱ δὲ θεσμοφύλακες δόντω[ν αὐτῶι]
[φυλὴ]ν καὶ τριττὺν καὶ χῶρον· *v v v v v* [? *vacat*]
[. . . .]ν τῆι βολῆι προσγράψαι πρ[. . . . [9] . . . .]
[. . . . [9] . . . .]ι[------

* Published in *BSA* 57 (1962), 1–4. For more recent work on Keos see E. Ruschenbusch, *ZPE* 48 (1982), 177–83; P. Brun, *ZPE* 76 (1989), 121–38.

[1] *BCH* 78 (1954), 316–22 (*SEG* xiv 530).

[2] The last thorough discussion seems to be that by Swoboda, *Sb. Wien* 199. ii (1924), 42–50. I do not note all points of disagreement.

This confirms Hiller's view that *thesmophylakes* and not *nomophylakes* were mentioned in lines 4–5 of the old inscription and makes it necessary to restore there [καὶ χῶρον] in line 6. Dunant and Thomopoulos give a revised text of lines 3–11,[3] but the unevenness of their line-length and the three later places in the old text where omission of words has been assumed make me wonder whether the whole inscription should not be restored with a rather longer line. There are, however, some lines which are hard to expand, and I offer no solution.

What do we now know of the institutions of federal Keos? From Tod 141.18ff.: ἐπι]μέλεσθαι δὲ ἐγ Κέωι [μὲν τήν τε βουλὴν καὶ τοὺς πρ]οβούλους καὶ τ[οὺς . . . . c. 12 . . . . καὶ τοὺς ἀσ]τυνόμους τῶ[ν Ἰστιαιέων we already had a rough list of officials, and, if the gap is really only of twelve letters, the now certain θεσμοφύλακας should probably fill it. We now see that Keos had a system based on φυλαί, τριττύες, and χῶροι. This is strangely complicated. A three-tier system is very suitable for populous territories like Athens or Hellenistic Samos, where outside the principal towns there were no outstanding units. For Keos, where there were four towns and nothing very much outside them, it is ridiculously complicated and artificial.

It would indeed be less complicated if we could assume that it functioned for three cities only. Each tribe then could have a trittys from each of the cities involved.[4] Such a view has substance as well as attraction. The evidence of *IG* ii² 43 (Tod 123) shows that Poiessa entered the Second Athenian Confederacy early by itself and was only followed some time later by Κείων Ἰουλιῆται, Καρθαιεῖς, Κορήσιοι (lines 82, 119–22).[5] It has generally been thought that || Poiessa did not appear with the other three on *IG* ii² 1128 (Tod 162).[6] Poiessa may well have gone its own way and not have been

[3] *BCH* 78 (1954), 319 (*SEG* xiv 531).

[4] So also Szanto, *Die griechischen Phylen* 49, but omitting Koresia, not Poiessa.

[5] For this question, nothing necessarily turns on the date of the accession of the three cities. But I have to record provisional disagreement with Woodhead (*AJA* 61 (1957), 371–3) and Sealey (*Phoenix* 11 (1957), 104–6), who place this group of names as having been brought in by Timotheus in 373. As they have both pointed out, the argument that this group of names preceded the Zakynthian entry is invalid and indeed probably wrong. Nevertheless, there seems to be a strongish argument against 373. No one will doubt that the Kean entry was made at the same time as that of Andros (line 112). In Skirophorion 374 the Delian amphictyony was reorganised and enlarged to readmit Andrian amphictyones beside the Athenians (Tod 125. 57ff.). I find it very hard to believe that Andrian entry to the Confederacy followed this reorganisation by a full year.

[6] See Tod ii, p. 183.

included in the federal framework. The *horoi* set up by Poiessa (*IG* xii. 5 568, 1100) late in the fifth or early in the fourth century point the same way.[7]

How old is this federal constitution? The people of the island are collectively Κεῖοι during the Persian Wars,[8] and Bacchylides ii with its reference to Keos' seventy Isthmian crowns points to sentimental unity,[9] but these facts are proved irrelevant by the strong probability that Ioulis continued to coin separately until about 465 and Karthaia and Koresia until about 450.[10] The evidence of the quota-lists is more difficult to evaluate. There we always find Κεῖοι, except on the occasion of the first recorded payment in spring 450, where we have Koresia paying 2¼ talents and Κεῖοι paying between 1 talent 1,200 drachmai and 1½ talents, against a steady payment for the whole group of 4 talents in the next three assessment periods. It is the opinion of the authors of *ATL*[11] that Koresia was not assessed separately but merely paid separately on this occasion, and that Keos was always assessed as a unit. This seems probable. But what kind of a unit? Was it merely a syntely for tribute-paying purposes or was it a federal unit? It is clear from the coin-evidence that Keos did not enter the League as a federal unit, and from the quota-lists that Koresia was capable of some independent action in 450, but it is obvious that the long line of Κεῖοι-entries in the quota-lists might conceal a change of organisation from syntely to federation at some point unknown. The only argument against such a possibility could be that the trittys-system would work better without Poiessa and that a separate Poiessa ought to appear in the quota-lists. This clearly will not bear much weight.

The rough *terminus ante quem* for the federation is furnished by the evidence of *IG* ii² 43 that Koresia, Karthaia, and Ioulis were associated in some way when they joined the Second Athenian Confederacy and by the evidence of the Sandwich Marble (*IG* ii² 1635.113 = Tod 125) that at some time before 377 the Κεῖοι had borrowed money as a unit. The first probably, the second certainly, implies the federal structure.[12]

[7] Cf. *Musée Belge* 25 (1921), 111. Swoboda thinks them compatible with a sympolity. But with a unified federal state?

[8] Her. VIII.1.2, 46.2; Tod 19 [= ML 27]; Paus. V.19.

[9] *SIG*³ 1057 is naturally relevant here, but it is not a contemporary document. Koerte (*Hermes* liii (1918), 118) puts it *c.* 400, which suits my argument well.

[10] E. S. G. Robinson, *Hesperia* Suppl. viii. 329. [11] iii.198.

[12] Swoboda (p. 43) denies the conclusion from Tod 125 and thinks the entries might have been combined when the accounts were put on stone, a procedure which seems neither useful nor likely.

I suggest that the clue to the date of the federation is to be found in its technical language, and that the new inscription provides it. The smallest unit in the constitution is the χῶρος or deme, and Dunant and Thomopoulos correctly point out that this name occurs only in one other place, Eretria. There it is known from *IG* xii. 9 189, for example, line 25: παρέχειν δὲ καὶ τοὺς χώρους ἱερέα κριτά, βοῦς, πάντα τὰ ἔτη, and I suspect that it lurks also in *IG* xii. 9 192. 10–11, where it would be simpler to read ἐπάρχεσθαι δὲ καὶ τοὺς χώρους, ἐκάστ⟨ου⟩ς τῶι Διονύσωι οἶνον καταπεμπομ[ένους] than to assume extensive mistakes in the copy and an unparalleled use of ἐπάρχεσθαι.[13] Dunant and Thomopoulos suggest that χῶροι in Keos go back to the time of Eretrian supremacy in Keos in the seventh century,[14] but, if this were so, one would have expected them to survive longer. Hellenistic Karthaia's citizenship-formula admits to φυλή and || οἶκος,[15] and Ioulis recognises no subdivisions of the city in its citizenship-formula, even though they must have existed, as we see, for example, from *IG* xii. 5 609. The citizenship-formula of the two federal inscriptions does not seem to have deep roots.

A rather less cogent pointer to Eretria comes from the πρόβουλοι of federal Keos. πρόβουλοι have their one appearance in Athens, and appear in scattered instances in Corinth and her north-western colonies.[16] In the Aegean their main home is Euboea, and apart from one Hellenistic appearance in Chalkis[17] and one highly dubious[18] and one Hadrianic appearance[19] in Karystos they are a characteristically Eretrian magistracy. They cannot in fact be shown to exist before a date towards the end of the fourth century,[20] but there is just no evidence before. In *IG* ii² 16. 6 (394 BC), for example, [προβούλους] is as possible a restoration as [στρατηγούς]. It does not seem unlikely that the Hellenistic πρόβουλοι of Eretria had classical predecessors. However, Hellenistic Koresia had πρόβουλοι (*IG* xii. 5 647), and they may be a genuine local institution.

The tribe–trittys–deme system of civic organisation is indeed not Eretrian. Wallace[21] has shown that in the Hellenistic period the demes were

[13] ἀπάρχεσθαι might be more reconcilable with χόρους. Cf. Robert, *Études épigraphiques et philologiques* 38–45.

[14] Strabo, x.1.10, p. 448. [15] *IG* xii. 5 540 and 1061. See Pridik, *De Cei Insulae Rebus* 59ff.

[16] Busolt–Swoboda, *Griechische Staatskunde* i.363–4. [17] *IG* xii. 9 207. 59.

[18] *IG* xii. 9 2. 2. [19] *IG* xii. 9 11 *passim*. [20] *IG* xii. 9 191.

[21] *Hesperia* xvi (1947), 114–46.

grouped in territorial districts, on a principle radically opposed to the Cleisthenic system. But we know also[22] that fifth-century Eretria had tribes which were an important part of the political system, and line 2 of the new inscription from Ioulis proves that classical Eretria divided itself by tribes and χῶροι.[23] The distinction between the Eretrian system and the system of federal Keos is merely that, to meet the needs of a federation of three cities aiming at unity, a leaf has been taken from the Athenian book and a trittys-system introduced.

If there is Eretrian influence in the federal constitution of Keos, its chronological place is clear. Eretria took the lead in the Euboean revolt from Athens.[24] It was in all probability the mint for the Euboean revolt-issue on the Aeginetan standard.[25] An Eretrian admiral fought with Lysander.[26] She allied herself with Hestiaia to the north when the Hestiaians returned.[27] It seems not unlikely that, with Spartan backing,[28] she took steps to guard her southern approaches by setting up a unified state in Keos with institutions modelled on hers and sympathisers in control. Koresia seems to have been the most wealthy city in 450, but the federal capital, as the find-spot of the inscriptions shows, was Ioulis. It was more central, and perhaps, as an inland city, had been less intimately associated with Athenian rule. It was certainly more troublesome to Athens in the fourth century.

When Keos returned to the Second Athenian Confederacy, the Athenian attitude to the federation was uncertain. The language of *IG* ii² 43 we have already noted. It is hardly whole-hearted recognition of Kean unity and may even be rejection of it.[29] Nevertheless, even at the later intervention (364?)

[22] *AM* lix (1934), 73; *Hesperia* v (1936), 273ff.; *IG* xii Suppl. 549.

[23] δῆμον is, of course, equally possible epigraphically. [24] Thuc. viii.95.

[25] Wallace, *The Euboean League and its Coinage* 1ff., 72ff.

[26] Paux. x.9.10; Tod 94 = ML 95, *g*.

[27] The date of this alliance is still unfixed. All that Thuc. viii.95 shows is that Athens did not lose Hestiaia-Oreos in 411. Lysander would certainly have restored the Hestiaians in 404, though there is no direct evidence in Xenophon or Plutarch. But they may have managed to return earlier. *IG* xii.9 187A and 188 present an interesting epigraphic phenomenon. They are by the same stonecutter (see *IG* xii.9, Tab. I), but by the time he comes to cut 188, he has lost or broken the tubular drill he used to cut the circular letters of 187A. The date of 187A is certainly 411; I would not put 188 many years later.

[28] If Dobree's conjecture in Dem. xviii.96 is right, as seems almost certain, Demosthenes asserts the presence of a Spartan harmost in Keos in 395. This raises no difficulties.

[29] Cf. Κεφαλλήνων Πρῶννοι (lines 107–8) and Ζακυνθίων ὁ δῆμος ὁ ἐν τῶι Νήλλωι (lines 131–4). But perhaps there is an implied contrast with Poiessa.

of Chabrias, the evidence about Kean unity is conflicting. For the || oaths themselves (Tod 142. 58ff.) refer always to Keos and the Keans as a whole, but they were originally set up, not only in Ioulis, where they were knocked down (line 31), but also in Karthaia (line 23). Furthermore, they are referred to in this inscription (that is, in 362) as oaths πρὸς τὰς πόλεις τὰς ἐγ Κέωι (lines 58, 69) as well as to the state as a whole (line 18). In 362 also there already exist separate στρατηγοί in Ioulis (lines 15, 44, 47) and Karthaia is described as a πόλις (line 54).

The formal close to this phase of doubt comes with *IG* ii² 404. 7ff. We begin with what seems to be the assertion that the cities entered the Confederacy individually:

[. . . . ⁸ . . . .] ἦλθον εἰς τὸ συνέδρ[ιον . . . . ⁹ . . . . τῶ]ι δήμω[ι . . . ⁷ . . .]
[. . . . ⁸ . . . . π]όλεις καὶ ἀν[εγ]ράφη [τῶν πόλ]εων ἑκάστης τὰ ὀ[νόματα ἐ]-
[ν τῆι στή]ληι καὶ σύνταξιν συντ[εταγμέ]ναι εἰσίν· δεδόχθ[αι τῶι δ]-
[ήμωι κυ]ρίους [μ]ὲ[ν] εἶν[α]ι τοὺς ὅρκ[ους κ]αὶ τὰς συνθήκας τῶ[ι δήμωι]
[τῶι Ἀθην]αίων, ἃς συνέθετο Χαβρί[ας σ]τρατηγὸς ὢν πρὸς τὰ[ς πόλει]-
[ς τὰς ἐν Κέ]ωι, καὶ πολιτεύεσθαι Κ[είου]ς κατὰ πόλεις κατ[ὰ τοὺς ὅρ]-
[κους καὶ τὰ]ς συνθήκας καὶ τὰ ψηφ[ίσμα]τα τοῦ δήμου τοῦ Ἀ[θηναίων]
[καὶ τὰς πόλ]εις τὰς ἐπὶ θαλάττηι ἐ[πισ]κευάζειν τὰ τεί[χη.

The Kean federation is dissolved, and for once we can observe and judge the argument of the proposer, whose name is lost. It is true that the name of each of the cities was inscribed on the stele, and this was a valid point, though not necessarily what was intended at the time of their admission. It is not stated but only implied that their assessment for *syntaxis* was individual, and this may not have been true. It is not true, as we have seen, that Chabrias' oaths provided for the break-up of the federation, though there had been preparation for this in Athenian decrees. Schweigert[30] has removed the only obstacles in the way of dating this specious decree to the beginning of the Social War, and this is unquestionably its proper place.[31] The federal

[30] *Hesperia* viii (1939), 14 n. 1, ix (1940), 322. Swoboda ends by giving the federal state a brief career in the 340s, largely by searching for occasions when Hestiaia, the partner in the old sympolity-treaty, was hostile to Athens.

[31] The numerous Κεῖοι in Delphic accounts present no obstacle, for the way an individual describes himself abroad is not evidence. I do not think that any of them can be proved to be later than 357, but it would not matter if they were. Swoboda agrees that they are irrelevant.

state had lasted for under fifty years and was not to be revived for another hundred. Created by Athens' enemies in Athens' darkest period, it could never be more than tolerated by her, and at the first sign of danger in the Aegean no promises of autonomy for the members of the Confederacy were likely to stand in the way of removing from her sea-approaches a constitutional mechanism which was a reminder of that period and those successful enemies.[32]

[32] This paper owes a great debt to T. B. Owen for general discussion and the removal of much doubtful matter.

5

The Athens Peace of 371

If we take an overall view, the drift of Xenophon's narrative seems fairly clear. After Leuctra it seems likely that Thebes will be preoccupied with Jason of Pherae and not in a position to interfere in the Peloponnese. The Athenians see that the Peloponnesians still think they ought to follow (*sc.* the Spartans) and that the Spartans are not yet in the position that they reduced the Athenians to (*sc.* in 404), and they therefore summon a conference (VI.5.1). This would lead us to expect that what follows will be aimed against Sparta and will provide guarantees for cities who wish to detach themselves from Sparta. This is confirmed by the appearance of the Eleans who are seeking a reversal of the terms of 400 (III.2.30) and the Mantineans who are seeking a reversal of their διοικισμός. Whatever the arguments are which are employed in the debate of VI.5.33–48, and I shall return to them, it is this debate which produces the φιλία with Sparta which has to be institutionalised later (VII.1.1–2). If Xenophon has suppressed Sparta's willing participation in the Athens conference and her acceptance as a partner by that conference, he has been seriously misleading on a point on which we would expect him to be clearest.

The first result of the conference is that the Mantineans regard themselves ὡς ἤδη αὐτόνομοι παντάπασιν ὄντες, and vote to rebuild. This will be a blow to the prestige of the Spartans, they send Agesilaus to prevent them from building, or at any rate from building without Sparta's permission, and he is unsuccessful; στρατεύειν γε μέντοι ἐπ' αὐτοὺς οὐ δυνατὸν

* Lewis notes that this was written in 1975, 'in a fit of irritation at the general acceptance which Sordi's contrary thesis had won, and was circulated for comment to various sceptics who continued resistant, particularly over Xen. *Hell.* VI.5.35–6'. He continued to believe in his view, and wanted it to be included in this volume.

For M. Sordi's view see *RFIC* 79 = n.s. 29 (1951), 34–64; cf. Ryder, *Koine Eirene* 71–2, 131–3, and more recently, e.g., R. Seager in *CAH* vi² 185–6. All references not otherwise identified are to Xen. *Hell.*

ἐδόκει εἶναι, ἐπ' αὐτονομίᾳ τῆς εἰρήνης γεγενεμημένης (VI.5.3–5). Two interpretations are possible which do not require Spartan participation in the Athens Peace: (1) the reference is to the Peace made at Sparta before Leuctra; (2) the reference is to the Athens Peace, and Spartan action is οὐ δυνατόν because Mantinea now has support which Sparta does not wish to provoke.

We now come to the trouble at Tegea. The Spartans decide to succour the Tegeate dead and exiles κατὰ τοὺς ὅρκους, and march against Mantinea because they had proceeded against Tegea παρὰ τοὺς ὅρκους (VI.5.10). The same ὅρκοι certainly, but these do not need to be those of the Athens Peace (we do not even know as yet whether Tegea was there). The argument may be that the Mantineans were bound by the Sparta Peace, or it may simply be a question of Peloponnesian League ὅρκοι.

In VI.5.33 Spartans arrive at Athens together with representatives τῶν ἔτι ὑπολοίπων συμμάχων (we can only name Corinth (37) and Phlius (38) specifically, though obviously Orchomenos, Epidauros and Pellene at least are still in line). The Spartan case is one of generalities from the past and stops after the first sentence of 35. The rest of 35–6 consists to my mind of things said by the Athenians in the assembly, and I cannot follow the view that ὁ πλεῖστος λόγος suddenly goes back to what the Spartans had said. τὸ μέγιστον of what they had said lay in their saving of Athens in 404.

36 says ὡς κατὰ τοὺς ὅρκους βοηθεῖν δέοι. This is unfortunately ambiguous. If it means Athens was bound, the only oaths which could bind her were those of the Athens Peace, and this would imply the participation of Sparta or Tegea or both in that Peace. It might however mean that they ought to implement the optional clause of the Sparta Peace. Dispute follows as to who was in the wrong.[1]

In 37 the Corinthian admits that this is disputable. However their own position is not. They have not lifted a finger since εἰρήνη ἐγένετο, i.e. since the Sparta Peace, but they have now suffered invasion. If Athens does not come to their help, they will be acting παρὰ τοὺς ὅρκους. καὶ ταῦτα ὧν αὐτοὶ ἐπεμελήθητε ὅρκων ὅπως πᾶσιν ὑμῖν πάντες ὀμόσαιμεν. There is no doubt this is a reference to the Athens Peace, which is the only one for which non-action will be a breach of oath. It seems to me to follow that this is the

[1] ἀδικεῖν (ἀδικησάντων σφῶν, §36) is a word that was used in the Sparta Peace: an optional sanctions clause states that anyone may support ταῖς ἀδικουμέναις πόλεσι and may not ally with τοῖς ἀδικοῦσιν (VI.3.18: in lectures over many years Lewis suggested reading εὔορκον . . . τοῖς ἀδικοῦσιν).

first time that these oaths have been brought into the discussion and that the ὅρκοι of §36 are those of the Sparta Peace. The Athenians accept the Corinthians' point. Prokles then returns to the general reasons of power-politics for helping the Spartans. There are appeals to χάρις and to self-interest, but nowhere the slightest suggestion that Athens has any legal obligation to Sparta.

What does the Corinthian reference to πάντες ἡμεῖς mean? Surely not 'all of us who have come now', but 'all of us who came then'. I agree that Corinthian participation weakens the case for the explicitly anti-Spartan nature of the Athens conference, but not, I think, totally.

Those who have Sparta in the treaty have some difficulty with the oath to ἐμμενεῖν τοῖς ψηφίσμασι τοῖς Ἀθηναίων καὶ τῶν συμμάχων. It is hard to think this means anything very different from e.g. *IG* ii² 97 = Tod 127, 33–5, τἄ]λλα ποι|[ήσω κατὰ τὰ δόγματα] τὰ Ἀθηνα[ί]ων κα[ὶ τῶ]ν || [συμμάχων], acquiescence in decisions arrived at by the mechanisms of the Athenian Confederacy. It seems very forced to argue that it is an oath to the general principles enunciated at the start of that Confederacy in 378, particularly when those principles were specifically anti-Spartan. It is perhaps not totally clear what relationship to the Athenian Confederacy was intended for the new Peloponnesian members, and their adhesion did not last long.[2]

[2] Lewis argued subsequently in Schachter (ed.), *Essays in the Topography, History and Culture of Boiotia* 71–3, that these arrangements were a model for an extension of the Boeotian League outside its territorial origins.

6

Preliminary notes on the Locri archive

Birthday presents should have an element of the unexpected about them, and, if I were to present Professor Klaffenbach with something Attic on this occasion, there would be more duty than pleasure about it, for both of us. I present him therefore with a holiday visit to Italian Locri, confident that the universality of his interests will induce him to overlook, at least in part, the dangers of the journey.

It is a tricky business to intervene with comments on a unified body of material, only half of which is as yet accessible, and which is being published with exemplary competence by A. De Franciscis,[1] who is clearly and rightly reserving discussion of some major aspects of the documents until he has published them all. My excuse is that I think I see one substantial point about the state-organisation which they reveal which seems worth making now. My remaining notes are relatively tiny observations on points of detail, which may help to supplement the editor's patient and well-judged commentary.

The material which we are considering consists of 38 or 39 bronze tablets, of which 19 have been published. It forms a unified archive concerned with the financial relations of the city of Locri Epizephyrii with the treasury of Zeus Olympios. It seems reasonably certain that we should call it a temple-archive, in the custody of the annually rotating board of three ἱερομνάμονες ἐπὶ θησαυρῶι, recording the occasions on which the city borrowed (or on one published occasion only, in no. 9, repaid) sums from the temple-treasure. Such transactions are normally authorised δόγματι βωλᾶς καὶ δάμω, much

* Published in *Klio* 52 (1970), 247–53. The definitive publication of all the texts is De Franciscis, *Stato e società in Locri Epizefiri*.

[1] *Klearchos* 3 (1961), 17–41; 4 (1962), 66–93; 6 (1964), 73–95; 7 (1965), 21–36. Full summaries in J. and L. Robert, 'Bulletin épigraphique', 1965 no. 494; 1966 no. 508; 1968 no. 603. De Franciscis gave further information about unpublished texts in *Congresso internazionale di numismatica, 1961*, ii. 117–29.

less frequently only δόγματι βωλᾶς. In three cases (nos. 4, 5, 32) the actual words of a decree appear, ordering, strictly, not the transaction itself, but its inscription on bronze. To state the position rather more dogmatically than De Franciscis, but not, I think, in conflict with his view as it has developed, the city has borrowed the money (ἁ πόλις ἐχρήσατο, or, in 12.9, ἐχρήσαντο, with Λοκροί the understood subject, as in 9.6, where they appear in the immediate context; no distinction should be sought between Λοκροί and ἁ πόλις). When, for purely psychological reasons, the drafting throws the weight on the *hieromnamones*, they are conceived of as lending it (ἐχρήσαμες 3.11, 14.3; ἔχρησαν 15.7, 17.8). One counter-example must, I think, be removed by emendation. In 7.9 ἐχρήσαντο || ἱερομνάμονες, with no word division on the tablet, would be more comfortably read as ἔχρησαν το⟨ὶ⟩ ἱερομνάμονες, a fairly trivial slip as errors on these tablets go; boards of officials in the nominative do not have to have the article, but it is permissible (4.10). De Franciscis has found himself forced to consider more complicated views of the transactions because of variations of practice in the naming of boards of officials, sometimes in the nominative, sometimes in the genitive. No importance should, I think, be attached to these variations, which reflect varying attitudes of the composer of the text to the degree in which the officials are regarded as active participants in the transaction or as dating and witnessing parties.

City officials regularly named are the eponym, whose office is not named, the πρόβωλοι or πρόβωλοι προάρχοντες and the πρόδικοι. In the published texts, the *probouloi* precede the *prodikoi* on sixteen occasions and the order is reversed only twice. The assumption that *probouloi* are senior and that one does not hold the office until one has been *prodikos* gives so far satisfactory results about the order of the tablets. More tentatively and more dangerously, one might guess that *hieromnamones* are more junior still (cf. nos. 13 and 17, where, on these assumptions, two *prodikoi* move up to become *probouloi* and one *hieromnamon* to become a *prodikos*). All inferences of this kind are insecure and probably premature. Other city-officials named more rarely are the ἱερομνάμονες ἐπὶ τῶι σίτωι (5), the τοιχιοποιοί (3), the ἐπιστάται (3); I should be inclined to guess that the ἄρχοντες of 4 and 33 form a group-designation rather than a single board.

The relation to the state or the temple of the φαταρχοί, operating in 8.6–7 through a προστάτας for a single month and their φατάρχιον (8.14) must for the moment remain obscure.

The letter-form dating will hardly take us closer than 350–250 BC. De Franciscis does not tell us how many eponyms are represented on the tablets. Those published so far give us twenty-one, which gives us a minimum span for the archive. Since he only has had occasion to report two duplications of eponym on other tablets, I take it that duplication is not very common and that we are dealing with a minimum span of nearer forty than thirty years. The most hopeful pointer to exact dates lies in the six tablets (1, 2, 13, 23, 30, 31) recording loans ἐς τὰν συντέλειαν Βασιλεῖ. Only Agathocles and Pyrrhus come into question. The direct evidence of connexion between Pyrrhus and Locri is strong,[2] and there are some numismatic arguments which also suggest him. The large number of tablets recording loans ἐς τὰν πυργοποιίαν or ἐς τὰν ὀχύρωσιν τᾶς πόλιος do not seem to me to give any very precise date. Unless we are very lucky with the other half of the archive, our chances of putting the eponyms and the tablets in any very precise order seem to be slim, but we may get some strongish indications of relative order. Again, speculation is premature.

There is, however, one aspect in which I do think we can make some progress, that of the social organisation of Locri. All boards of officials, except perhaps || the ἐπιστάται (3.13–14), come in threes, 'o perché erano magistrature di tre membri ciascuna, oppure perché più probabilmente era una tema tratta dall' intero corpo di quei magistrati che si occupavano di simili atti amministrativi'.[3] De Franciscis does not explain his preferred solution, which is of course perfectly possible in itself. However, I think it can be shown that the other solution is to be preferred. All officials in these tablets have as part of their normal designation a three-letter abbreviation preceding their name, for which we can adopt De Franciscis' tentative designation as a 'demotic'. This is known as a South Italian practice, of which the most familiar parallel is at Heraclea (*DGE* 62–3), though we are mercifully spared here the symbol-designation, e.g. ἄνθεμον, which at Heraclea intervenes between the abbreviation and their name.[4] Observation and tabulation show that these demotics always appear in the same position in a group of three; e.g. on all twelve occasions when officials with the demotic Θρα appear, these officials stand first in their group of three. This pattern is

[2] P. Lévêque, *Pyrrhos, passim*, especially 497–501. [3] *Klearchos* 1 (1961), 39.

[4] Most recently treated by G. Pugliese Carratelli, *Atti e memorie della Società Magna Grecia* n.s. 6/7 (1965/6) 209–14; A. Uguzzoni & F. Ghinatti, *Le tavole greche di Eraclea* 125–7.

certain; many other demotics appear in unvarying positions on anything up to nine occasions. We can confidently assert that boards of three are three in number because the state is divided into three units which we may designate as tribes, and that these tribes had a standard official order.

We are now in a position to tabulate the demotics by tribes. The numbers in brackets give the total number of occasions on which the demotic has so far appeared, not merely the number of instances where the tribal affiliation is guaranteed. I add some scattered information about possibly relevant proper names.

Tribe I

Ανα	(1)	
Ανξ	(3)	The Volscian Ἄνξωρ (Diod. Sic. XIV.6.5) is too far away.
Γαψ	(6)	
Ευρ	(5)	
Θρα	(13)	This demotic appears as Θρασ in 14.2, probably by error, but two members have names formed on the root.
Κοβ	(4)	
Λακ	(6)	Λακέρεια πόλις Ἰταλίας, Λακίνιον ὄρος Κρότωνος (St Byz.).
Ὁμβ	(6)	Ὁμβρικοί?
Σκι	(3)	Σκίδρος?
Τυν	(5)	
Ψαθ	(5)	

Tribe II

Αστ	(3)	Ἀστροβόλιον· ἐν Ἰταλίᾳ (Hsch.)
Βοω	(2)	
Δυσ	(8)	||
Κρα	(3)	Κρᾶθις.
Κυλ	(6)	
Μνα	(7)	
Σκα	(2)	Σκάρφεια brought from home?
Σωτ	(9)	
Τηλ	(7)	cf. Τῆλυς the tyrant of Sybaris.
Τιω	(8)	Τιώρα (Dion. Hal. *A.R.* 1.14.5) is a very long way away, but this Aboriginal name may be a parallel.

Tribe III

Αγα	(1)	
Αγκ	(7)	Ἄγκαρα πόλις Ἰταλίας, Ἀγκύριον πόλις Ἰταλίας (St. Byz.)
Αγφ	(2)	
Αλχ	(7)	
Γαγ	(6)	
Λογ	(3)	
Πετ	(2)	Πετηλία.
Προ	(7)	
Πυρ	(7)	
Στρ	(2)	
Φαω	(9)	

The total of demotics so far attested is 32, distributed II in Tribe I, 10 in Tribe II, II in Tribe III. Statistical methods exist[5] for using the distribution of the number of instances of known demotics to predict the likely range within which the total number of demotics will fall. Since the unpublished material will double the sample, there hardly seems any point in bothering a statistician at this stage. I have had the guess hazarded that the prediction would be close to 40, and this is the sort of possibility which we ought to reckon with. Yet I cannot stop myself being attracted by another approach. The two tribes with II demotics each has one of its demotics so far attested only once. It is clearly possible that, in Tribe I, Ανα may turn out to be another form of a syncopated Ἀνξ and that, in Tribe III, Αγα, Αγκ and Αγφ may be reducible to two. The name Ὄναιος in Αγα (8. 3) does recur in Αγκ (3. 3); less seductively, Γνάθις appears both in Αγκ (14. 6) and Αγφ (7. 6). This would give us a situation in which there were three tribes, each with ten subunits, and this would ring some interesting bells. But the statistical probabilities are certainly against such a solution at the moment. We are clearly still far from the Hundred Houses and the Thousand of which the literary sources speak.

The abbreviated 'demotics' are all pronounceable, and in this respect distinguished from the ΖΝΞ which appears at the bottom of No. 15 and which is well explained by De Franciscis as the initials of the three *hieromnamones*. One could not dispute a view that our information is so limited that the

[5] Cf., e.g., M. Thompson, *The New Style Silver Coinage of Athens* 711.

abbreviations || may conceal patronymic or gentilician units. My own feeling, however, is that Λακ(ίνιον), Σκί(δρος), Κρᾶ(θις) and Πετ(ηλία) form a sufficient nucleus of likely local names to make it probable that De Franciscis' designation, demotics, is on the right lines.

A general problem is raised by the form in which the eponyms' names are given. The regular beginning of a tablet is *demoticum* ἐπὶ *nomen* and this order also appears in the run of a sentence: ἐκ τῶ ἐνιαυτῶ τῶ Θρα – ἐπὶ Ἡρακλήτω (1.2–3), τὰν χρῆστιν τὰν Τηλ ἐπὶ Εὐθυμίδα (4.8–9), τοὶ ἄρχοντες τοὶ Φαω ἐπὶ Νεοδάμω (4.10–11). There is some prosopographical confirmation for the assumption that the demotic is appropriate to the eponym, but the word-order is so unGreek that it is difficult to be satisfied with the assumption that we simply have here an equivalent of the Attic ἐπὶ Ἀλκαίου Σκαμβωνίδου ἄρχοντος or ἐπὶ Ἀλκαίου ἄρχοντος Σκαμβωνίδου. It may be worth hazarding the suggestion that the demotic is in some sense part of the designation of the year, that Herakletos' year was the year of the 'deme' Θρα, for whom Herakletos performed the eponymous function. The parallel which suggests itself is the designation of a year in the Gortyn Code (v.5–6) as ἃι ὅκ' ὁ Αἰθ[α]λεὺ(ς) σταρτὸς ἐκόσμιον οἱ σὺν Κυ[λ]λῶι. It would be hazardous to go further and wonder whether there was a cycle in which all demes named a year before any recurred. Our 21 known years show only two recurring demes.

Scattered observations

No. 3. I would interpret the boards of *probouloi* and *prodikoi* in the genitive (though five of the six names are in the nominative) as almost purely dates. There then follow the *hieromnamones* in the nominative, the *toichiopoioi* in the nominative, τούτοις ἐχρήσαμες . . . , *epistatai* in the nominative, τούτοις ἄλλα . . . On the lines of my opening analysis, I take this as describing two payments, both made by the *hieromnamones*: the first to the *toichiopoioi* the second to the *epistatai*.

Nos. 4 and 5 are two decrees from Euphrainetos' year. In the first, the *hieromnamones* are instructed to write on bronze τὰν χρῆστιν τὰν Τηλ ἐπὶ Εὐθυμίδα τῶ σίτω τῶ ἐπακτῶ τὰν τίμαν τὰν οὐκ ἀπέδωκαν τοὶ ἄρχοντες τοὶ Φαω ἐπὶ Νεοδάμω. In the second, the instruction is given to write τὸ μείωμα τᾶς τιμᾶς τῶν πυρῶν καὶ τῶν κυάμων τᾶς τιμᾶς. We then get separate figures for wheat and beans and a total, followed by the note τοῦτο

ὀφήλει ἁ πόλις τῶι Διί. It seems to me that the simplest explanation is this. The foodstuffs are bought with money advanced, not as yet formally loaned, by Zeus. The city resells them at a loss, but puts back immediately in Zeus' treasury such proceeds as there are. The difference, described as μείωμα in 5.7 and τὰν τιμὰν τὰν οὐκ ἀπέδωκαν τοὶ ἄρχοντες in 4.10, then has to be formally acknowledged as a loan. In the case of the transaction of Neodamos' year, this formal acknowledgement did not take place, the money did not become a χρῆστις, until Euthymidas' year. Even then it was for some reason not inscribed, and the inscription is not ordered until the year of Euphrainetos. In that year, the *hieromnamones*, having learnt || wisdom, arrange for the whole procedure of the current year to be carried out at once.

In 5.10, the figure for the κεφ(άλωμα) should be transcribed as 𐅄 𐅄 𐅄 𐅄 𐅄 T T | Σ Σ Δ Λ Λ Λ | | | | (142 talents, 2 staters, 13 litrai, 4 unciae). This is a straight misprint. The commentary is correct.

No. 9. This is the solitary repayment, and it takes the following form. 'The Lokrians paid back the money which they had borrowed by decree of the council (in the current year) (183 talents, 3 staters, 13 litrai, 4 unciae) καὶ ἀπὸ τὰν ἀρχαίαν ὀφηλάν (617 talents, 11½ litrai, 2 unciae). τοῦ τάδε ἀπέδωκαν χαλκῶ σταθμὸν τάλαντα (160) λίτρας (17) ποὶ τὰς θύρας τὰς χαλκέας τὰς τῶι Διὶ τῶι Ὀλυμπίωι ἐξέδωκαν ποιῆσαι ἐν τὸν ναὸν ἐπ' ἀμφότερα τὰ ζεύγη. τὸ τάλαντον τῶ ὁλκῶ ἕκαστον ἐν ταλάντοις πέντε ἀργυρίω.

Several points call for comment. De Franciscis reports that the tablets contain no single debt of 617 talents, 11½ litrai, 2 unciae, but it is not at all probable that ἀπὸ should take the accusative in any case and it would certainly be better to accentuate ἀπὸ τᾶν ἀρχαίαν ὀφηλᾶν. I suggest that it might be more profitable to look at the transaction the other way round from the way in which the Locrians put it and not regard the handing over the bronze as a way of satisfying specific debts, but as a transaction which is being carried out anyway and which can be made to reduce the state-debt. De Franciscis has not done the sum, but once done, it makes the situation clear. The total of the two repayments is 800 talents, 4 staters, 5 litrai. This is precisely 5 times the weight of the 160 talents, 17 litrai of bronze, for De Franciscis has shown elsewhere that 20 litrai = 1 stater.[6] Therefore, the meaning of τὸ τάλαντον τῶ ὁλκῶ ἕκαστον ἐν ταλάντοις πέντε ἀργυρίω must be 'counting each talent of the weight of the bronze as 5 talents of

[6] *Congresso internazionale di numismatica* (see page 247 note 1) 122–5.

silver', and we need not consider other possible meanings of ὅλκος. I suggest that the quantity of bronze was the primary element in this transaction. It is being handed over to Zeus and is given a money value, which is used in the first place to offset a current debt, the surplus being set vaguely against the old debts.

ἐπ' ἀμφότερα τὰ ζεύγη. De Franciscis interprets as 'both halves of a pair of doors', appealing to well-known uses of ζύγον. I have to discuss doors elsewhere and confine myself here to saying that ζύγον in this context probably means a 'panel'. I see no difficulty in translating 'for both pairs (of doors)'.

No. 13. There are more months to come, so it would be premature to discuss the calendar. But we should note that Ἀγρήιος is not quite new, either absolutely or in a Lokrian context, for Ἀγρῆος was read by Daux *BCH* 63 (1939), 166 (see Lerat, *Locriens de l'Ouest*, ii. 240) in *SGDI* 2136 (Phalika) and is there equivalent to Delphic Theoxenios.

In 1612–13 the sub-units after the 63 medimnoi look more like H ᚴᚴ than ᚴᚴᚴᚴ. As the surrounding calculation suggests that the sub-units should constitute ⅔ of a medimnos (1563 + x medimnoi × 1 st. 10 l. = 390 T. 5 st. 10 l.), we must interpret H as a *hemimedimnos* and ᚴ as a *hemihekteon*. De || Franciscis' reading, on the assumption that ᚴ is a *hekteus*, gives the same result.

Notes of this kind, confined to disagreements, are inevitably misleading. In this case, they convey an entirely false impression about the work De Franciscis has put in to these documents, concealing both the large quantity of solid fact he has elicited from these texts and his patient discussion of problems which still await answers. We look forward to his publication of the remainder of the texts eagerly and with confidence.

7

Temple inventories in ancient Greece

The task I have been given is to introduce you to some of the textual evidence for Greek metal-work. It is extremely copious and, particularly when it deals with gold and silver objects, it goes some way to making up for the loss of whole categories of objects which do not normally survive. With isolated exceptions, it has been neglected; my impression is that it has not really been explored by those interested in the development of metal-work, and that much remains to be done in correlating with real objects or representations of them.

My brief is to talk about inventories, that is, comprehensive documents drawn up by the responsible officials to list and publish the articles in their care.[1] But, long before any text of this kind survives or is likely to have existed, one does get the odd text where individual donors commemorated their dedication with a separate inscription on stone or bronze. The practice is slightly different from writing the dedication on the object itself or, in the case, for example, of a statue, on its base. As the sixth century BC goes on, the text can become fairly explicit. The stele from Sigeion in the British Museum records the gift by Phanodikos of Prokonnesos to the citizens of Sigeion for use in the prytaneion, that is, for public entertainments, of a mixing-bowl, a stand and a strainer; the material is not specified. It has recently been suggested that the gift was thought so magnificent that Phanodikos was given

* Published in M. Vickers (ed.), *Pots and Pans* (1986), 71–81 (Faculty of Oriental Studies, Oxford). Note 1 here has been transferred from the Bibliography there; notes 2–27 here were notes 1–26 there, and have been edited to match the style of this volume.

[1] The fifth-century Athenian texts have had a recent edition in *IG* i^3, fasc. 1 (1981), nos. 292–362, with summaries on pp. 305–6, 318, 351–2; the fourth-century texts are in *IG* ii^2, fasc. 2.i (1927), nos. 1370–1496, with important addenda in fasc. 2.ii (1931), pp. 797–810, but much has changed since then, not all of which has been published, and I have been sparing in annotation.

the more than human status of a hero.[2] Towards the end of the century, in Samos, two citizens of distant Perinthos dedicated to Hera as a tithe a gold Gorgon, a silver Siren, a silver phiale and a bronze lamp-stand, and added that the whole dedication, including the setting up of the stone, had cost them 212 Samian staters.[3]

Inventories as such start in the fifth century and are surely to be connected, like many other kinds of publication which start then, with a spread of democratic institutions and the concept that officials were responsible to the people. I would not like to say precisely when and where this starts, but the main story begins for us in 433 in Athens, and the motive is || more than purely administrative; it is financial. Pericles saw the gold and silver objects in Athenian temples and shrines as part of the city's reserve capital; even the gold which went into Pheidias' chryselephantine statue of Athena was of known weight and made to be removable, though it survived many financial crises before being melted into coin at the beginning of the third century. It was with the intention of putting Athens' financial machinery in order to face the coming Peloponnesian War that the treasurers of Athena were ordered to prepare regular annual inventories of what was in their charge, distinguishing gold and silver, weighing and counting the existing stock and any new dedications of the year, and these were checked as they left office.[4]

These inventories were published on stone, and, considering the hazards of time, we have a remarkably complete collection of them covering the next hundred and thirty years. Patterns of display changed. For the first forty years or so there were separate stelai for each of the three main relevant rooms of what we call the Parthenon, but later they were consolidated into mammoth texts. By the 360s the annual stelai were 2 metres high, 1.10 metres across and a quarter of a metre thick, and could run to four hundred lines of text in letters 5 millimetres high. This could hardly go on; there was some attempt to reduce the amount of material which had to go on stone each year, and the letters get smaller and smaller.

Points of detail remain obscure, but we have a fairish picture of the vicissitudes of Athena's treasures over those hundred and thirty years. As the Peloponnesian War drew to a close, the lists became shorter and shorter as

[2] Richter, *Archaic Gravestones of Attica*, no. 53; L. H. Jeffery, *8th Epigraphical Congress* i.52–4.

[3] G. Klaffenbach, *MDAI* 6 (1953), 15–20. [4] ML 58 (cf. 76); Fornara 119 (cf. 143).

familiar objects disappeared into the melting-pot for Athens' first gold coinage. In the fourth century there is again a steady accretion of objects commissioned for public cult, dedications by officials and private individuals and crowns received from foreign states wishing to honour Athens and Athena. From time to time, we see traces of systematic operations to melt down damaged or small objects and replace them by larger ones. The biggest of these was under Lycurgus in the 330s, who revived Periclean policies and increased the stock of the jewellery and equipment of the basket-carriers of the Panathenaic procession from thirty sets to a hundred. But magnificence could not last, and the last of all the lists, from a troubled year round 300, certainly appears to list all the stock and then to end: 'these we did not hand over to our successors'.[5]

At this point, the continuous Athenian information breaks off and is only patchily supplied by the occasional account of the remodelling of the offerings of individual shrines; one of these I shall come back to. Its place is supplied for the next hundred and fifty years or so principally by Delos. Inventory-making || had started there in the fourth century while it was under Athenian administration. It continued during Delian independence and produced texts far longer than anything we have from Athens. I confess that I do not know my way about these texts very well and they are even less exploited than the Athenian texts, except by one devoted researcher who pursues all their references to different types of coinage. The task of tackling them is not made easier by the fact that they still lack any kind of published index.

It should be clear that there is potentially a good deal of information of all kinds to be got out of these texts. I do not ask this audience to share my enthusiasm for the minutiae of Athenian administration or finance or for the sometimes invaluable diplomatic information provided by the crowns from foreign states. There is, however, religious information, not only about the type of offering thought appropriate to each god, but about the gods themselves. Such controversial importations as Bendis or Ammon get at any rate *termini ante quem* from the inventories.

There are weights. The convention was to weigh silver objects to the nearest half-obol, a bit over a third of a gram, gold objects to the nearest quarter-obol, half that. An unpublished Oxford dissertation[6] has argued

[5] S. N. Koumanoudes and S. G. Miller, *Hesperia* 40 (1971), 448–57.

[6] C. H. Grayson, 'Weighing in Ancient Greece'.

that this was collective delusion and that no contemporary weighing technique was capable of anything like that accuracy, but the Athenians were evidently happy with it. It is clear that there were standard weights for various objects. Silver hydriae aspired to a weight of 1,000 drachmae (435 grams), silver phialae to one of 100 drachmae. Silver oenochoae were in the 400–600 drachmae range, drinking cups (*poteria*) 25 to 40. I have made no attempt to calculate standard dimensions. Other silver objects observed less of a standard. Around 370 BC, for example, Athena possessed three platters (*pinakes*), one described as large at 2,027½ drachmae and two others at 1,028⅛ and 719½, and two lavers (*chernibeia*), at 1,040 and 940 drachmae.

Gold objects, at first sight, present an extraordinary variety of weights. The explanation is to be found in the fact that the Athenians thought in terms of a silver standard. When, as they frequently did, they ordered that someone be honoured with a gold crown for 500 drachmae, they did not mean that the gold crown itself would weigh 500 drachmae, though the mistake has been made by very eminent archaeologists, but that its cost would be 500 silver drachmae, a very different matter. Attention to the likely cost of gold objects is therefore not unprofitable, and I have in my time produced a tentative sketch, on the basis of the inventories, of the ups and downs of the gold-silver ratio between the relatively fixed points of 14:1 in the 430s and 10:1 in the 330s.[7]

That the inventories and their accessions can be chronologically ordered is || clearly of importance, and it should on occasion be possible to determine at least when some names and descriptions come into use or are first reported. I have mentioned some names already, many of them pot-shapes as well, and you will be eager to know what I can do for you by way of descriptions. To me they only rarely seem exciting, and the fullest and most valuable of all the descriptions which the treasurers of Athena have left us is not of a separate object at all, but of the doors of the Hekatompedon, i.e. the Parthenon, where some of the metalwork had broken away and lost some bosses by the 340s. The description is sufficient to allow us to recognise the false stone door of a Macedonian tomb in Istanbul as a direct copy of the original doors.[8]

[7] Lewis, in *Essays in Greek Coinage . . . S. Robinson* 105–10.

[8] *IG* ii² 1455.36–49, 1457.9–20 (the missing word in these texts must be *pompholugon*); Michaelis, *Der Parthenon* 316; Mendel, *Musées Impériaux Ottomans: Catalogue* i, no. 138.

Fifth-century descriptions are rarely very full, and perhaps the general picture is more interesting. In 433 the Pronaos of the Parthenon held 'the gold phiale from which they make the libations', 104 silver phialai, three silver horns (*kerata*), three silver cups (*poteria*), and a silver lamp. Virtually all its accessions for the next twenty-five years fall into these categories, though one new cup in 427/6 is specifically described as Chalcidian, whether that be an ethnic adjective or a type, and there is a growing number of *argyrides*, a very enigmatic word – (their weights vary from 60 to over 200 drachmae).

The inner room, the Hekatompedon, had much more gold. In 433 it held a gold kore on a stele, unweighed, and a silver sprinkler, also unweighed, and two gold phialai weighing 1,344 drachmae between them, to which in that year another was added, weighing 1,200 drachmae by itself. If you work that amount out in silver, you find that it would have kept a trireme at sea for three months. In subsequent years, the prevailing accessions are gold crowns, one specifically for the Nike on the hand of Pheidias' statue, but there is a little silver, particularly a heavy incense-box (*thymiaterion*, 1,000 drachmae); the *argyrides* of the Pronaos are matched, at more or less the same dates, by *chrysides* here.

What the Athenians called the Parthenon, one of the rooms at the back, is much more fun. There were some straight gold pieces in 433, a crown and some phialai, and there were silver phialai as well, but otherwise it is a strange miscellany, of no great intrinsic value. Most of it survived for the next seventy or eighty years. There was a gold *carchesion*, said to be a cup with a waist, with a silver bottom; this belonged to an out-of-town shrine of Herakles. There are two silver-gilt nails (as they weighed 800 grams between them, I cannot imagine what they were for), a silver-gilt mask and various other plated objects. The nicest of these was an imitation corn-field with twelve shoots. There were famous parallels to this at Delphi, and there are surviving || examples.[9] Some of the contents lie right outside our subject, various bits of armour and furniture and some lyres. The fifth-century accessions also tend to be miscellaneous, except for some gold crowns. The suppression of the revolt of Mytilene in 427 leaves some trace, not only in the spoil, a shield, a bronze Illyrian helmet and three silver cups (*kotylai*), but in a strange present from loyal Methymna, a gilt ivory flute-case (*sybene*).

[9] Plut. *Pyth. Or.* 401 F; one was sold at Sotheby's some time ago, but I cannot remember the metal.

Some quite full descriptions come in. I pick out a pair of accessions datable to within a year or so of 408, two gold necklaces adorned with stones, each with twenty roses. One had a ram's head and weighed 30 drachmae, the other is described as smaller and was evidently slightly different.

In all these rooms, practically everything meltable went in the last years of the war, together with seven of the eight golden Nikai.[10] The Pronaos in particular was emptied, with only a solitary gold crown left.

The first substantial accession of the post-war period came in 402/1 when twenty silver hydriai were made, almost certainly the processional vessels (*pompeia*), which we know to have been made from the confiscated property of the Thirty Tyrants.[11] This is a new shape for silver; Athena had possessed none before. It became a fairly regular one; by 370 thirty are listed for Athena Polias. I have already said that there was evidence that Lycurgus raised the number of basket-carriers from thirty to a hundred; perhaps in 370 there were thirty hydria-carriers as well. It is in any case likely that these silver hydriai are replacing pottery ones; hydriai are carried on the North Frieze of the Parthenon.[12] The switch from pottery to silver evidently spread to other cults. By 370 Athena Nike has four, Artemis Brauronia seven, the Dioskouroi three, Demeter and Kore five, and Aphrodite one.

I doubt if further investigation of chronological developments would be helpful within this short paper, and it would be better to pick out some fourth-century objects.

Some large objects with gold and silver on wooden frames had evidently survived the war. One gold basket adorned with ivory animals weighed over 2,000 drachmae, and there was a similar censer. In the early fourth century the Athenians went over to a bronze core for such objects. Very close to 400 we get two pairs: baskets, gold over bronze, one adorned with a Zeus, the other with an Apollo, weighing over 3,500 drachmae apiece; censers, gold over bronze, weighing nearly 3,000 drachmae, distinguished only by the leaves on one being straight, on the other curved. Where precisely the fullest || description fits in, I don't know. This was a censer, silver over wood,

[10] These, the grandest of all the offerings, had two talents' weight of gold apiece: see A. M. Woodward, Ἀρχ. Ἐφ. 1 (1937), 159–70, D. B. Thompson, *Hesperia* 13 (1944), 173–209.

[11] M. B. Walbank, *Hesperia* 51 (1982), 74–98 at 97–8.

[12] It is a well-known problem that the frieze depicts men, while the literary evidence speaks of women (Simon, *Festivals of Attica* 60, 63–5); the silver hydriai weighed well over 4 kilos apiece, but girls should have continued to manage that.

weighing 1,448 drachmae and adorned with a Nike. It had a separate silver cover, weighing 58 drachmae.

For the next topic, I would like to reverse the procedure and start, more or less, from some objects. One shape where the original relationship of pot and metal has always been clear is the phiale. Richter and Milne[13] take as their type phiale a pot in Boston of which they reasonably say that its ribbing imitates metalwork. Gold and silver phialai are common in literature, turning up even in private houses, and, as we have seen, they existed in quantity on the fifth-century Acropolis. There were possibilities for the shape in metalwork which did not exist in pottery, and there are traces of specialised descriptions even in the fifth century.[14] In the fourth century, the picture is a good deal richer. As far as real objects are concerned, my prime concern is with two gold phialai; there is other related material. One will be familiar from its appearance at the British Museum in 1976. It is from the Panagyurishte Treasure.[15] Its base has three rows of negroes' heads, with an inner row of acorns. The other is in New York;[16] it has three rows of acorns and an inner row of nuts. It has no known provenance, but, besides a Greek graffito, it has a weight inscription in Punic, which no one has done very well at reading; it is the lighter of the two, weighing 747 grams.

Bulgarian scholars have claimed the Panagyurishte phiale for Lampsakos, and have repeated this as a matter of fact in various places. There is nothing whatever to this ascription, which rests on a false interpretation of the weight inscriptions. It has two of these: one of them just says H (100), the other HΡΔ Δ Δ Δ ΡΗΤ (or something very like it), 196 drachmae and a quarter-obol. It actually weighs 845.7 grams. Venedikov argued at a very early stage that the aim of the inscription was to distinguish between a weight in staters and a weight in drachmae.[17] He even thought that the weight in drachmae was really appropriate to silver and anomalous for gold. Assuming that the '100' inscription proved a stater of 8.457 grams, he looked no further than Lampsakos. The truth is much simpler. The '100' represents

[13] Richter and Milne, *Shapes and Names of Athenian Vases* fig. 181.

[14] There was already a silver phiale with a gold boss or navel at Eleusis around 420: *IG* i³ 385.1–2.

[15] Venedikov and Gerassimov, *Thracian Art Treasures* figs 131–2; British Museum, *Thracian Treasures from Bulgaria* no. 361.

[16] D. Von Bothmer, *BMM* n.s. 21 (1962/3), 154–66; Strong, *Greek and Roman Gold and Silver Plate* 97–8 with pl. 23A.

[17] I. Venedikov, *A. Ant. Hung.* 6 (1958), 67–86 at 82–6.

darics, as Kahn[18] correctly saw, though without citing the clear fourth-century evidence for objects being weighed in darics.[19] It has always been pretty clear that the other weight inscription, besides being Attic in appearance, made perfect sense as an Attic weight, with a drachma of 4.313 grams.

The general view is that this phiale is late fourth century. I am not in a || position to dispute it, but the motif is certainly older. You will not find the phrase *phiale Aithiopis* in Liddell and Scott or its supplement, but it occurs. Kontoleon pointed out that Athena possessed in the 370s four gold *phialai Aithiopides* weighing 805½ drachmae as a group, very similar in weight to the Panagyurishte piece.[20] I am beginning to suspect that the similarity is due to their also having been made to a hundred-daric standard; the hypothesis explains some peculiar Athenian weights. It may be argued that the standard counts against their Athenian origin, but there is no other reason to suppose that the group was not made at Athens around 380; there is no indication of a foreign dedicator. I am far from sure that the Panagyurishte phiale was not made in Athens. According to Pausanias (1.33.3), Pheidias' statue of Nemesis had long before carried a phiale with Ethiopians.

What about the Metropolitan piece with its acorns and nuts? Liddell and Scott does have the phrase *phiale balanote*, but only from a fragmentary statement in Athenaeus (XI.502B). There we are told that it was so called because there were gold knuckle-bones (if there is what *astragaloi* means here) on its base. He goes on to quote Semos as saying that in Delos there was a golden palm-tree, dedicated by the Naxians, and gold *phialai karyotai* (with nuts). Delos does indeed provide us with some firmly dated evidence. In a text of 364/3 under Athenian control (*Inscriptions de Délos* 104) we find six *phialai chrysai balanotai* weighing 1,190⅓ drachmae, seven *phialai chrysai leiai* (smooth), weighing 983¾ drachmae (much lighter), two *aktinotai* (with rays), weight lost, and one *karyote* (with nuts), dedicated by the Naxians, weighing 195 drachmae. The *balanotai* and the *karyote* fit into the weight range we have seen at Panagyurishte and Athens, and indeed the Delians were not very good at distinguishing acorns from other nuts, these *phialai*

[18] H. A. Kahn, in E. Simon, *AK* 3 (1960), 3–29 at 26–8.

[19] *SIG*³ 168, 276, the second even by Athenian colonists on Samos; *F. Delphes*, iii.5, no. 61.ii.A1–8.

[20] N. Kontoleon, *Balkan Studies* 3 (1962), 185–200, with slight inaccuracies. There was another group along with them, six *phialai ptilotai* (with wings), weighing 1,036 drachmae, a good deal lighter.

balanotai appear in later inventories as *karyotai*. Again, only the Naxian dedication takes us outside Athens or Athenian-controlled Delos, but the weight of the New York piece and its Punic inscription suggest that it comes from rather further afield, though it may have been made by a Greek whose name began with Pausi-.

We do not know how long the *phialai balanotoi* had been at Delos in 364/3 and there is some evidence for a longish history for the type. It has been claimed[21] that the Erechtheum caryatids are carrying such phialai, and they may go further back. A fragment of the comic poet Cratinus, whose last play was produced in 423, refers to *phialai balaneiomphaloi* (fr. 54 Kassel–Austin). Hellenistic scholars explained this by saying that the boss looked like the hearth of a bath-house, but, as Cratinus' latest editors remark, without knowing much of the evidence, acorns are more likely to be relevant than || bath-houses. We have no actual acorn-shaped bosses as yet.[22] We can perhaps look forward to them; we can certainly hope for wings and rays. Contrariwise, we might eventually learn the name of the phiale from Kul Oba in the Hermitage with bearded heads, gorgons, panthers and dolphins.[23]

As the world expanded, more exotic objects will have come to swell Athena's store. There had long been one or two pieces of Persian spoil, not fully described. It is a great pity that the full description has not survived of a series of presents made by Alexander's Queen, the Bactrian Roxana; we can only distinguish a golden rhyton, a term making its first appearance in the inventories, and a gold neckpiece; more was said about them, but it is off the stone. Similarly, I have spent endless hours trying to decipher just what it was with ivory feet and bronze supports which was dedicated by Euthydike, the Athenian descendant of Miltiades the victor of Marathon,

[21] Strong, *Greek and Roman Gold and Silver Plate* 81.

[22] That acorn bosses existed is suggested by another group of phialai which came into the inventories around the middle of the century (*IG* ii² 1443.143–53: see Lehner, *Ueber die athenischen Schatzverzeichnisse*, 113–14). The metal is not certain: the weights are enormous, apparently 1,200–1,400 drachmae. Three appear to be described as having the little sphinx on *the* acorn, three as having the little sphinx on *the* nut, one as having the little sphinx on *the* apple, one as having the palladion on something slightly different, perhaps a knuckle-bone. These would clearly make better sense as referring to the shape of a central boss rather than to numerous articles arranged in rows. The *phiale balanote* is not only a gold shape; there is a bronze one (*IG* ii² 1425.357).

[23] See *BMM* n.s. 32. v (1973/4), no. 83 with pl. 20 (the weight of this piece is 698.55 grams); *Or des Scythes* no. 94.

mistress of Demetrios Poliorcetes and wife of a dynast of Cyrene, forerunner of a long series of great Hellenistic ladies.

In the later lists, for almost the first time, we start getting a few names of craftsmen, but there is not much to say about them. At least one, Nikokrates of Kolonos, was an Athenian citizen; others, Kittos, Herakleiodoros and Platon, were probably not. Some of the names are presumably hereditary. In the 330s there is one with the unattractive name of Mys. He will have been named after the craftsman who made the shield of Pheidias' Athena Promachos to Parrhasios' design.

More light on fourth-century metal workers comes from Delphi, and goes some way to demonstrating the importance of Athens in the craft. In the 330s Delphi faced the task of replacing the major offerings melted into coin by the sacrilegious Phocians, notably the great gold and silver bowls dedicated by Croesus. Apparently all the possible contractors were selected from Athens, six of uncertain origin, though one of them may be the same Nikokrates, two certain Athenian citizens and four Corinthians.[24] They stayed for twelve days to survey the problems and make their bids, receiving a ration allowance while on site.[25] Whether we are seeing the actual craftsmen rather than || contractors is perhaps doubtful. The sums involved were very substantial, nearly 5 talents for replacing the silver bowl which held 600 amphorae, and the money was paid over only by instalments.[26]

In this case, of course, they had to find the metal as well. Most of our Athenian evidence relates to remaking from metal objects provided for the purpose. For the fourth century we are more or less confined to some rather fragmentary accounts which only cover losses in melting. In one such case the goldsmith was given 2,950 drachmae weight of gold objects. 14 per cent of this was lost in the melting, leaving 2,531 drachmae of pure gold, but a rake through the ashes produced another 116 drachmae, reducing the loss to 10 per cent. I am sure that there is more to be found, but I think that we have to wait for the end of the third century in Athens for a really detailed account to give the workman's pay (*IG* ii^2 839. 80–8). This is one of the remelting operations I referred to earlier. In the shrine of the Hero Physician there were thirty-three small dedications of models of various parts of the body;

[24] There is a problem about one of these, Theomnestos, later (see n.26) referred to as a Sicyonian.

[25] *F. Delphes*, iii.5, no. 48.23–41. [26] *F. Delphes*, iii.5, no. 62.5–9.

eyes and thighs are specified. These weighed 116 drachmae, but there was also a phiale weighing 100 drachmae and 18 drachmae in cash. The five citizens and the public slave who supervised the operation reported that, as instructed, they had spent 15 drachmae on a sacrifice to appease the Hero. The melting of the 216 drachmae of objects produced a 6 per cent loss. 8½ drachmae were reserved for putting up the stele with the accounts. The workman of the new oenochoe got 12 drachmae. They were left with an oenochoe weighing 183½ drachmae and 2 drachmae over. With this they promised to make another model. Whether a making charge of 7 per cent was anything like standard, I cannot tell,[27] and I would not like to venture a cost of living figure for the late third century. What is perhaps significant is that the craftsman is not named. We should not exaggerate the social importance of routine metal work.

[27] But see my article (n. 7), 108, where I calculated a 6⅔ per cent making charge for a phiale in 434.

8

Democratic institutions and their diffusion

I shall try to define, from an epigraphic viewpoint, the effects that the two periods of Athenian political hegemony in the classical period and Athens' more continuous and longer cultural hegemony may have had on the Greek world as a whole. The subject could hardly be more important, but it is in some ways very neglected. The only major work I know in which the topic of the diffusion of Athenian institutions is a principal theme is Swoboda's *Griechische Volksbeschlüsse* of 1890, a work remarkable not only for the immense labour which must have gone into it but also for its general good sense.

Swoboda was entirely concerned with decrees, and they will also be my main concern. They provide the bulk of the evidence. As Finley points out in an article to appear in *Annales*, there is simply nothing, outside the major sanctuaries, to match the amount of administrative and factual detail which Athens found it necessary to put on stone. This fact is in itself significant and has something to tell us about the political climate of states outside Athens. I only happen to know one text, from Ephesos *c.* 300 BC (*Inschriften von Ephesos* 4. 20–4), where publication of documents is ordered so that ὁ βουλόμενος τῶν πολιτῶν may know, in this case, about the sale of land. The question of what it was thought necessary to publish extends to decrees themselves. The topic must be studied both chronologically and by subject. Swoboda complained of the infrequency of texts earlier than the Hellenistic period. We can surely now say that the phenomenon is not due to the accidents of discovery, but is a real deficiency, also amounting to a fact in itself. As far as subject is concerned, we surely have a sample sufficient for statistical purposes. The calculations have not yet been made, but they will certainly show that the proportion of honorary decrees to all decrees outside Athens is much higher than that for Athens, with the obvious qualification

* Published in *8th Epigraphic Congress* i (1984), 55–61. This paper foreshadows Rhodes with Lewis, *The Decrees of the Greek States*. The article by M. I. Finley referred to in the second paragraph is in *Annales (ESC)*, 37.5–6 (1982), 697–713: see especially p. 705.

that the quantity of published decrees outside Athens increases at about the same time as the proportion of honorary decrees in Athens itself begins to rise.

I begin with the purest epigraphical topic, the question of the outward appearance of decrees, a topic which Swoboda was not in a position even to touch. The question is that of the ability to display a longish text on stone and of thinking that this is a proper thing to do. Since we are dealing with the classical period and with Athens, we are in practice talking about the diffusion of the *stoichedon* style. A re-examination is needed. || Austin's classic work is now nearly fifty years old. The hardest work went into Athens and he missed some fairly important texts outside it. His final conclusion was that the origin and the main developments of the style were Attic, that in Athens alone could its development be plotted, and that in the few Greek cities outside Attica in which the style was freely used, this did not come about until after Attic *stoichedon* had reached its zenith.

This judgement may now require some considerable modification. I think that there was always more to say for Curtius' view that the style had Ionian origins than Austin allowed. Austin missed two very early texts from Erythrai quite comparable with anything Attic[1] and the Perinthian dedication in the Heraion[2] improves our knowledge of developments on Samos. But what concerns us more is the ability to produce long texts in the fifth century. Precise dating is generally difficult, but the middle two quarters of the fifth century show long texts from Argos, Erythrai, Miletos and now Teos,[3] which are certainly comparable to, say, *IG* i³ 6 on the Mysteries, and show no obvious sign of being influenced by Athens.

Nevertheless, there surely is some influence to be detected. Consider first two fifth-century proxeny decrees, in which the influence is not visible, *Inscriptions de Délos* 71,[4] *stoichedon*, but independent of Athens, and a very amateurish piece of work from Eretria.[5] Then turn to some slightly later texts, three from Lindos[6] and a decree from Miletos.[7] As we shall see, there is more to link them with Athens than their mere appearance, but their

[1] *Inschriften von Erythrai und Klazomenai* 209, 321. [2] *SEG* xii 391.

[3] Argos: ML 42. Erythrai: *Inschriften von Erythrai und Klazomenai* 2. Miletos: ML 43. Teos: Herrmann, *Chiron* 11 (1981), 1–30.

[4] Surely *c.* 450, rather than 'dernières années du V^e siècle ou des premières du IV^e' (Plassart). Photograph, *BCH* 33 (1909), 473.

[5] *IG* xii Suppl. 549. Photograph, *Hesperia* 5 (1936), 274.

[6] *Lindos* ii 16, 16 App., *Clara Rhodos* ix 211ff. [7] *Klio* 52 (1970), 163.

outward show is certainly Attic. There are states in the fourth century, notably Erythrai, where the tradition is virtually indistinguishable from that of Athens.

Austin suggested two main channels of influence. People went to Athens, saw what texts were like there, and demanded the same standards at home. Some Athenian decrees were exported and spread the influence. I think I am more inclined to see importance in the movements of individual craftsmen, and find the ability to produce a decent text more rare than is sometimes supposed. Long ago, I identified an individual working at Eretria in the early fourth century,[8] and my impression is that, at Erythrai in the fourth century, there was only one public stonecutter at a time.

I turn now to the actual texts and begin with the beginning, the prescript. Swoboda only knew one text where Athenian influence was totally clear, an inscription from Kyzikos still only known from a very bad copy.[9] It clearly began simply ἔδοξεν τῶι δήμωι, not known in Athens before 400, but continued with the tribe in prytany, the epistates, grammateus and proposer, all with patronymics. No other text of Kyzikos has anything like || all these elements, though one early fourth-century text which has survived[10] has an epistates (who happens to be called Athenaios). Swoboda rightly pointed out that Kyzikos continued to have tribal prytaneis and prytaneis serving for a month; uniquely outside Athens, it has the financial board kolakretai. The obvious difference from Athens is that it retained the six tribes of its mother-city Miletos. This degree of conformity with Athenian models certainly suggests either Athenian intervention or an ostentatious assertion of loyalty. The text has not attracted the attention of historians of the Athenian Empire, but we should perhaps recall that Kyzikos' early tribute record is patchy and that she is totally absent from the third assessment-period. There may have been a substantial Athenian intervention in Kyzikos in 443/2.

Much more attention has been paid to her mother-city, Miletos. To start with the tribes, at some date after 448/7,[11] the six earlier tribes which we find at Kyzikos were replaced by a twelve-tribe system which lasted into imperial times.[12] We can now name seven of these, five of them identical

[8] *BSA* 57 (1962), 3 n. 27. [9] Michel 533, revised by Perdrizet, *NC* 1899, 1–4.

[10] *BCH* 13 (1889), 515.

[11] *SIG*³ 57. All Milesian dates in this passage follow Cavaignac, *REH* 90 (1924), 311–14, rather than Rehm's standard chronology.

[12] Rehm, *Milet* i. 3, 159 n. 1.

with Athenian Cleisthenic tribes, plus Theseis and Asopis. The conjecture that all ten Cleisthenic tribes were there may be correct. Remodelling under Athenian influence is certain, but, although there is a fair amount of evidence and the date ought to be in the 440s, the precise sequence of events is not yet established. That the reorganisation also left a mark on Milesian prescripts has been known since 1901, when a decree of 380/79 was published,[13] showing eponym, month, Κεκροπὶς ἐπρυτάνευεν, Φιλιννῆς Ἡροδότο ἐπεστάτει, ἔδοξεν τῆι βουλῆι καὶ τῶι δήμωι, Ἡράκλειτος εἶπεν. The order is unAttic, but the Attic influence was clear; it had lasted into a period when Miletos was well outside Athenian control, though Milesian prescripts after Alexander were indeed different. In 1970 Peter Herrmann produced an even more Attic example,[14] we have already noted its Attic outward appearance. The beginning is lost, but we have, besides the date, Λεωντ[ὶς ἐπρυτά]νευεν, Τήλαγ[ρ]ος ἐγρα[μμάτευ]εν, Τήμεν[ος] ἐ[πεστ]άτε, [. . . ⁶ . .]ς εἶπεν. Herrmann considered three dates: 435/4, 411/10, or 403/2. Political arguments surely exclude 411/10, though perhaps not 403/2; 435/4 is clearly the most likely date. It is tiresome that we do not have the full prescript. Herrmann was certainly wrong even to consider ἔδοξεν τῶι δήμωι, since line 28 ἢν δόξει τῶι δήμωι proves that we are dealing with a probouleuma. It will have started ἔδοξεν τῆι βουλῆι or ἔδοξεν τῆι βουλῆι καὶ τῶι δήμωι.

No other state has yet produced tribe ἐπρυτάνευεν, but another look at the late fifth-century texts of Lindos, of which we have already noted the Attic appearance, is not unprofitable. *Lindos* ii 16 App., certainly from Naukratis, has ἔδοξε τᾶι βωλᾶι καὶ τῶι δάμωι· Δέσπων ἐγραμμάτευε, Ἀρχεάναξς εἶπε. *Clara Rhodos* ix 211ff. has even more: [ἔδοξε τᾶι βωλᾶι καὶ τῶ]ι δάμωι· Οι[. . . ⁶ . .] ἐπεστάτει, Σ[. . . ⁷ . . .] ἐγραμμάτευε, Ἀγ[άθ]αρχος εἶπε,[15] and eventually in its main text attests the presence of prytaneis || with a one-month term. *Lindos* ii 16 is probably rightly held to show the adaptation of these models to an oligarchic constitution, running [ἔδοξε τᾶι β]ολᾶι ἐπὶ π[ρυτανίων τ]ῶν ἀμφὶ Δει[νίαν.

Swoboda pushed the investigation of prescripts further. He was inclined to see a certain limited Attic influence in texts which had a close association between a named epistates and a named proposer. He thought this mainly a phenomenon of the Cyclades. It remains fairly rare, but extends rather

[13] *Sb. Berlin* 1901, 911 = *SGDI* 5496 = Sokolowski, *Lois sacrées d'Asie Mineure* 45.

[14] *Klio* 52 (1970), 163–73. [15] A similar text at Paros: *IG* xii.5 123.

further than that, most noticeably, in a rather different shape, at Argos. I am rather more struck by the rarity with which we find a grammateus in the prescript. I doubt if I have a complete collection, but I have very few indeed and they tend to be unique even for their own state.

But of course the most substantial dividing line in prescripts is between prescripts which name no proposer at all and those which do; the latter class may be subdivided between those which have named proposers and those which name a board of magistrates. This is a topic which takes us far beyond any question of direct Athenian influence; many states with individual proposers were subject to Athenian influence, but some of them were not.

What are prescripts for and what are the realities behind them? Swoboda attached a good deal of importance to their dating function and thought that that might be influenced by Athens. I see little in this, and dates do not seem to me to appear at Athens noticeably earlier than elsewhere. There are two more substantial points about these Athenian-style prescripts. Firstly, the naming of an individual fixes responsibility. The γραφὴ παρανόμων has not yet appeared outside Athens under that name, but there is ample and widespread evidence for the legal responsibility which might attach to those who put an improper proposal on the agenda, those who spoke for it and those who put it to the vote. Swoboda could already collect seventeen states with such Strafformeln, and the list can now be considerably expanded. In fact, there is not much correlation between these states and either Athenian influence or the naming of epistatai and proposers. The point must rather be that all states recognised such a responsibility, but for some the responsibility was collective, for others individual. I suspect that we can recognise at least a tendency for the latter category to be more under Athenian influence.

The second point lies in the implications of the prytany system. I suggest that a prytany system is required when there is thought likely to be a good deal of business for the boule and when the boule is relatively large and needs subdivision. It need not necessarily be under Athenian influence. There are, after all, two early examples of ἐπιμήνιοι, one at Eretria[16] where it is a tribe which is thus described, and one at Miletos,[17] immediately before the Athenian-style democracy, where we do not know how the ἐπιμήνιοι were selected. However, I think we can say, on fairly a priori grounds, that, if Athens saw it in her interests to encourage democracy in a particular state,

[16] See note 5. [17] ML 43; cf. Piérart, *AC* 38 (1969), 365–88.

she ought to have preferred a relatively large boule and some machinery to make sure that there was a fairly continuous turnover in the people who had their hands on the levers of government. All this quite apart from the natural tendency to believe that Athenian institutions were best. ||

However, there is no particular reason to believe that Athens did aim at any great uniformity. We tend to speak too glibly about Athens' encouragement of democracy, without asking what was involved, and we do not get a great deal of detail on the mechanism. A great deal could have been done, and no doubt was done, simply by control of personnel, making sure that undesirables left the city concerned and that Athens' friends were protected. The measures needed to make things safe for Athens will have varied, and we may overestimate the extent to which Athens intervened in detail. Let us look at the case where we have most detail, Erythrai,[18] where I accept a date of 452 for Athenian intervention. The provisions are remarkably simple. There is to be a boule appointed by lot; it is to have 120 members. That seems on the small side. Even more surprisingly, there is no indication of how the 120 are to be made up; Erythrai may have had three tribes,[19] but did the Athenians know? We then run into difficulties, but there was clearly an age-limit of thirty, and sitting on the boule more than once in four years was forbidden. The oath which the boule is to swear concentrates on loyalty to the Erythraean πλῆθος, Athens and her allies, resistance to exiles and guarantees for the existing citizens. There is not a word here about how the boule is to organise itself, what it is to be allowed to do without reference to the πλῆθος or how business is to be managed. There is of course to be an Athenian garrison-commander, and it may be expected that he will make suggestions. But I do not think that we should impose our own ideas of what kind of institutions the Athenians will tolerate, and this is what has generally been done. Though Engelmann and Merkelbach betray slight unease, they have followed the communis opinio that the internal Erythraean constitutional document *Inschriften von Erythrai und Klazomenai* 2 must be earlier than 452 because of its 'non-democratic' features. I see no epigraphic attractions in a date as early as this, and certainly do not think that the (very small) property qualifications for dikasts and the small jury are matters that the Athenians would have taken notice of. Unfortunately, though there are

[18] ML 40 = *IG* i³ 14.

[19] Pausanias VII.5.12 not cogent; there are nine *strategoi* in the Hellenistic period.

prytaneis in this text, I do not think that we can fix their term of office or be sure that the four-month period later prominent at Erythrai is yet operative. In any case, these prytaneis have left no mark on the Erythraean decree-prescript.

It can of course be reasonably objected that 452 is relatively early in Athenian imperial development and that control might have been more rigorous later. I do not myself take this view, and am impressed, for example, by the fact that, when the Athenians appoint ἐκλογεῖς τοῦ φόρου in 426,[20] they are interested in their responsibilities to Athens, not their behaviour in their own states.

It would take too long here to examine what was finally Swoboda's main claim for Athenian influence on the decrees of other states. He held that what became the standard shape of the Hellenistic honorary decree, an ἐπειδή . . . clause outlining the honorand's claims, a ὅπως οὖν . . . clause expressing the reasons for making a decree, followed by a motion formula and the honours themselves, was essentially an Athenian invention. I admit to being doubtful about this. As far as I can see, although all the elements exist || before, we do not get Athenian decrees of quite this shape until after 307, and there are non-Attic decrees of this shape pretty close to that date.

I want to close with what seems to me a more important topic. It is common ground, as expressed, for example, by A. H. M. Jones in *The Greek City* and adopted by de Ste. Croix in his new book, *The Class Struggle in the Ancient Greek World*, that, after Alexander, Greek states more or less all claimed to be democratic, starting their decrees ἔδοξεν τῇ βουλῇ καὶ τῷ δήμῳ, but that, in the Hellenistic period, the democratic formulae covered a steady shift to a position in which virtually all decrees were formulated by a limited number of persons, generally officials, and the demos merely assented to them. I am sure that this picture is essentially true, though closer analysis will show considerable fluctuations and variations. The closer analysis is of course still a field for research. It forms, for example, the whole subject of Helmut Müller's stimulating *Milesische Volksbeschlüsse*, although I cannot accept his central contention about the meaning of γνώμη ἐπιστατῶν, and of some clear pages in Susan Sherwin-White's *Ancient Cos*. It is of course difficult to get to the realities. Dr Sherwin-White points out that, at Cos and at other places, including Miletos, there are texts where we have no

[20] ML 68 = *IG* i³ 68.

mention of the boule at all, which begin ἔδοξεν τῶι δήμωι and continue δεδόχθαι τῶι δήμωι. But before we accept these as traces of the purest democracy, there are two qualifications to be made. Peter Rhodes has pointed out[21] that there are similar cases in Athens where we can in fact see that there has been a probouleuma in essentially the same terms, so that the formulae may be an incomplete guide to procedure. Secondly, in the very rare cases (I know of less than a dozen) where these formulae are accompanied by an individual proposer, we cannot be sure what kind of man he is and in what capacity he is making the proposal. I find it significant that, in two of these rare cases, at Cos (Paton–Hicks 10; cf. *SIG*[3] 568) and at Erythrai (*Inschriften von Erythrai und Klazomenai* 24; cf. 28), even our limited information shows that the proposer is a very wealthy man.

But what of the period before Alexander? If it is accepted that, in the Hellenistic period, democratic formulae may conceal the domination of the few, how can we determine whether earlier democracies were really places of full discussion or simply the cloak under which relatively small groups used the power of Athens to ensure their own position? The techniques we have for answering this question are very limited in their scope, but the question must be asked. I have already suggested that the very fact that we do not have much material may be significant about the realities of public life. On the other hand, it would be legitimate to say that those states which paid their assemblies (we know of Rhodes and Iasos) are likely originally to have been influenced by the wish to have wide participation. Beyond that, we can only look at the material for pointers. Essentially, what we are looking at is the range of people who make proposals, a matter which I have already touched on, and the extent to which official proposals and probouleumata may be amended in the assembly. Swoboda was not much interested in amendments. He only knew of four non-Athenian examples, and said that this scarcity was surely just a matter of what was reported and published. There is certainly something in this. We have no amendment-formulae from Paros itself, but two late Parian examples in dossiers published || by foreign states which omitted no detail. But there is still an enormous discrepancy. From Athens down to the year 321 we have 52 amendment-formulae. From the whole of the rest of the Greek world at any date, my count is 9 (I exclude those which simply refer to a previous decree or law), all from places which

[21] *The Athenian Boule* 67.

had had heavy Athenian influence.[22] Some of these, particularly from the fourth century, are certainly substantial, but three of them, two from Paros and one from Miletos, follow open probouleumata and are moved by members of the boule; the phenomenon of amendments being moved by bouleutai is not unknown at Athens. It would not be unreasonable to see these cases as vestiges of an earlier procedure by which real amendment in the assembly was possible, but, as far as I can see, we are never in a position to say that any given amendment was moved from the floor. For that matter, we do not know in how many states outside Athens a private citizen could speak from the floor.

Epigraphists are concerned with what was published. The officials charged with publication were, consciously or unconsciously, trying to give an impression. Fortunately, they sometimes included material which enables us to probe deeper, and we have a duty to do so. Aristotle has warned us (*Politics* iv. 1292B15) that there are states which have the legal framework of radical democracy, but which in their way of life and their practices are, rather, oligarchic. It will not detract from our appreciation of the classical age of Greek democracy if we scrutinise it closely to see how far its realities corresponded to its ideals.

[22] Erythrai: *Inschriften von Erythrai und Klazomenai* 10. Miletos: *Milet* i.3 139. Cos: Sherwin-White, *Ancient Cos* 115 n. 169, cf. 181. Arkesine: *IG* xii.7 38. Ilion (?): *Inschriften von Ilion* 55. Halikarnassos: Michel 452. Paros: *SEG* xxiii 489. 22ff. and b15; *SIG*[3] 562. Thasos: Pouilloux, *Recherches sur l'histoire et les cultes de Thasos* i 205ff.

9

Public property in the city

The standard books on the Greek city either have no treatment of public property at all or take it for granted in treating public finances. This is an attempt to fill some of the gap. It is concerned mostly with classical Athens and operates with a rather narrow definition, pursuing the key Greek word for 'public', *demosios*.[1] It will emerge in the course of the paper that other forms of communal ownership operate functionally in a very similar way, in that the city can exercise control of their administration and revenues.

In the Appendix, I review some current views about the history of the word *demos*, and conclude that it can, very early and certainly before the word *demosios* starts appearing, simply mean the whole citizen body with no programmatic nuance of 'lower classes' or implications of democracy.

The earliest relevant appearance of *demosios* is in Solon fr. 4, the unjust *hegemones* (leaders) who steal and snatch, sparing neither sacred nor public property (*outh' hieron kteanon oute ti demosion pheidomenoi*, 12–13). It has been suggested to me that there may be some elements of persuasive definition here, with a transition from the property of individual members of the *demos* to that of the *demos* as a whole, but I incline to think that the lines do establish the concept of public property for Solon's time, as well as the use for it of the word *demosios*; we may also recall the statement, generally passed over, that Solon's *seisachtheia* involved the abolition of debts, both private (*idia*) and public (*demosia*) (Arist. *Ath. Pol.* 6.1). There is of course no reason to think the usage specifically Athenian; we can recall the athlete who might get corn from the public possessions of || the city (*sita . . . demosion kteanon ek poleos*, Xenophanes fr. 2, 8).

Solon's language happens to contrast *demosia* with *hiera*; to *hiera* we shall

* This was published in O. Murray and S. Price (eds.), *The Greek City* (1990), 245–63. (Reprinted by permission of Oxford University Press.) Lewis expressed thanks to Sally Humphreys, Robin Osborne and the editors for help in the revision of the paper.

[1] There are others, notably *koinos* (common).

return. We should note that later, at least from the fifth century, *demosia* are most commonly contrasted with *idia* (private), not only adjectivally, but in the adverbs *demosiai . . . idiai*. For reasons which are by no means clear to me, when the contrast is with *hiera*, it is more normal to use *hosia* (profane) than *demosia*, both in technical and non-technical contexts.

What are the origins of public property? If we simplify the origins and purpose of the *polis*, we can perhaps give it three primitive functions.

The role of common defence may not need money/property at first (walls are more a matter of labour), but is going to involve it as soon as the cost of the equipment needed (including, for instance, both ships and mercenaries) outruns the resources of individuals.

In the religious sphere, leaving buildings on one side for the moment, we might say, moving rapidly over some very rugged ground, that the community needs resources as soon as it moves in on or adds to the cults already being performed by family groups. These resources are not sacred in their origins, but are used to supplement the existing resources of the cults. The keyword here is *demoteles*, applicable both to sacrifices and to festivals.[2] I have no example of the word earlier than the fifth century, though I suspect it existed earlier.[3] So far we have covered the two main things which the archaic state spent its money on; compare the Peisistratidai, who out of their 5 per cent tax carried on their wars and sacrificed sacrifices (Thucydides VI.54.5). Admittedly, they also adorned the city fairly, but public building does not, I think, rank as a primitive function of the *polis*.[4]

The third primitive function is the administration of justice. || Except for eccentric states who found it desirable to pay their juries, this cost no money. What it surely did do was to provide one obvious way in which the *polis/demos* could acquire property. Here the history of confiscation is important.[5] In a sense, confiscation is a capital punishment, associated – as it was, for example, in the list of penalties referred to an Athenian court for

[2] Sacrifices, Her. VI.57.1 (Sparta), Orac. *ap.* Dem. XXI.53; festival, Thuc. II.15.2 (Athens, Synoikia). See now J. K. Davies, *CAH* iv². 379.

[3] The practice could be expressed in other ways. Note the Salaminioi in 363/2 (*LSCGS* 19.20–1, 86–7): ὅσα μὲν ἡ πόλις παρέχει ἐκ τῶ δημοσίο, . . . ξύλα ἐφ' ἱεροῖς ἡ πόλις δίδωσιν ἐκ κύρβεων.

[4] Temple-building, indeed, may be an interest of the collectivity, but will always have been classified as *hieron*, not *demosion*.

[5] I am not much concerned here with the history of the fine, surely very primitive, and of course possible long before the introduction of coinage.

the fifth-century allies – with death, exile, and loss of civic rights. Someone is being excluded from the community, and the question inevitably arises as to what happens to his property. I do not propose to spend time on the theoretical political regimes in which there may have been no private property, no alienability of land, only *genos*-property. As strictly defined as this, they are incompatible with the confiscation of the property of individuals.[6]

Confiscation-words always seem to involve the *demos*-root. *demeuo* is the commonest verb, but there are isolated instances of *demosieuo* and *demosioo*. *demosion/demosia einai/gignesthai* (be or become public) are very common indeed.

The earliest alleged instance of confiscation which I have so far managed to recall is that of the Bacchiads of Corinth in 657. In the anti-Kypselos story of Herodotus, v.92 ε, Kypselos exiled many Corinthians, deprived many of their property and a very large number of their life. There could be other ways of describing the event, and Nicolaus (*F. Gr. H.* 90F57 §7) has 'he exiled the Bacchiads and confiscated (*edemeuse*) their property'.[7] Let us throw in a couple of sixth-century examples: Peisistratos' property was auctioned by the *demosios* (presumably a public slave acting as herald) and bought by Kallias, the only person prepared to bid (Her. vi.121.2); at Naxos, Lygdamis found no one prepared to give much for the property of those whom he had exiled, and sold it back to the exiles (Arist. *Oeconomicus*, ii.1346b7ff.; there is a great deal missing from this story).

There are clearly various possibilities. As far as Corinth is || concerned, Will argued,[8] without evidence, that the Bacchiad land was distributed to the landless, and the same thing has sometimes been supposed to have happened at Athens, where confiscation by Peisistratos himself is not actually attested. Such situations constitute a redistribution of land, and create no permanent accession to the city's own property. Immediate resale of confiscated property was always common, and we have detailed evidence for it at Athens, above all with the confiscated property of the Hermokopidai and the Thirty. It is doubtful whether such resales ever did much to build the capital structure of the city. Periklean Athens was abnormal in carrying capital balances over from year to year. The normal Greek attitude did not

[6] Cf. Lewis, in *Ancient Society and Institutions . . . V. Ehrenberg* 181–2.

[7] '. . . may be nothing more than a restatement in fourth-century terms' (J. B. Salmon, *Wealthy Corinth* 195).

[8] *Korinthiaka* 477–81.

distinguish between capital and income. Resale of confiscated property was straightforward reprivatisation, to allow the proceeds of confiscations to balance the income and expenditure account for the current year (cf., for fourth-century Athens, Lys. xxx.22.) And, of course, there is always a strand of Greek thinking in which the city, faced with a windfall, may simply declare a dividend to its members; for Athens and for confiscations, I can only think of the property of the mining magnate Diphilos, and the story of the distribution of his property is not all that well attested ([Plut.], *X Or.* 843D).

The possibility most relevant to us is that the confiscating city sees for the property confiscated either an actual practical use or a means of ensuring income. A situation akin to confiscation may arise at the end of a tyranny.[9] It is probably the case that the Athenian tyrants' property in the silver mines passed to the Athenians collectively, but otherwise I do not think I know a demonstrable Athenian case, and, in our fullest fourth-century texts about the retention and leasing of confiscated property, the property is in the hands, not of a state, but of the Delian and Delphian Amphictionies.[10]

What other means were there for the state to acquire property? At Athens, there seem to have been various legal conventions, || the origins of which are uncertain. I do not pretend to understand the entire legal situation about the silver mines, but it seems relatively clear that the state assumed the right to lease the use of what was underground.[11] I suppose that it was the case that the state owned the major quarries, but this was not, apparently, a universal rule; the deme of Eleusis in 332/1 can lease quarries which are sacred to or belong to Herakles in Akris (*SEG* xxviii 103). The concept of ownership may be inappropriate.[12] Similarly, it is not clear what the rules were about areas which were simply vacant, with no obvious claimant (*erema*). Xenophon in the *Poroi* (2.6) asserts that, in the 350s, there were many vacant houses and plots within the walls and recommends giving the right of land-ownership (*enktesis*) to worthy metics who are prepared to build on them; nothing is said about the nature of the state's rights in the matter or about money passing.

To this we can add those areas which had been in the public domain

[9] A tyrant's objects of practical use may go for sacred purposes (e.g. Her. III.123.1).

[10] *Inscriptions de Délos* 98B 31ff.; *Fouilles de Delphes* iii. 5, nos. 15–18.

[11] See, e.g., R. J. Hopper, *BSA* 48 (1953), 200–54.

[12] On quarries, see R. G. Osborne, *Demos* 93–110.

for so long that no question of private property could arise. The Agora, the Kerameikos, and the Pnyx are the obvious examples; the various gymnasia may fall somewhere between public and sacred property. We find all these delimited by boundary-markers (*horoi*) without the use of *demosia*. One building, the prison, is distinctive enough actually to be called *to demosion*. Another category of importance is the road-system. It seems that it was generally recognised that roads were public property. We have *horoi* demarcating them and they are constantly named as boundaries; the main text which explicitly describes a road as *demosia* is Demosthenes LV, at 13 and 16.

What other pieces of real property can we see? The fifth century does not provide very much.[13] There is the publicly owned house (*oikia demosia*) named as a boundary for the surface water to which the lessee of the shrine of Neleus/Kodros/Basile will be entitled (*IG* i³ 84.36; 417 BC). This was certainly in Athens, apparently just inside the city-wall to the south of the Acropolis (Travlos, *Pictorial Dictionary* 332 with || fig. 435). There is some reason to think that it may have been near the law-court at the Palladion, and it was surely large enough to serve as a clear landmark, but there is absolutely no indication as to its use.

But the greatest quantity of fifth-century evidence for public property is concerned with the Piraeus. What the status of the relevant land was before the Piraeus was planned we cannot know, and we have no indication of how the state went about acquiring it to implement the ideas of Themistokles and Hippodamos of Miletos.[14] Hippodamos is extremely relevant to our subject at the theoretical level. He apparently recommended (Arist. *Pol.* II.1267B33ff.) that a city's territory be divided into three, *hiera* to provide the resources for religious observance, *demosia*, or *koine*, from which the military class could live (Aristotle does not report how these two divisions would be worked), and *idia*, for the farmers. I doubt if this is directly relevant to his operations at the Piraeus. It now seems that his main innovation there was, not the grid-system of streets with which modern scholars have associated him, but which is certainly older, but the concepts of *nemēsis* and *diairesis*, the systematic allocation of different parts of an area for different purposes. We have a large number of fifth-century *horoi* from the Piraeus. I

[13] The public bath-house and other public property discovered by Hiller in *IG* i² 385 have disappeared in *IG* i³ 420.

[14] On Hippodamos, see A. Burns, *Historia* 25 (1976), 414–28.

have no very clear ideas about their date or which of them can or cannot be specifically associated with Hippodamos; there is a fair amount of literature about whether epigraphic rules about three-bar sigma and tailed rho should apply to such *horoi* and whether Hippodamos can have survived to make a town-plan for Rhodes after 408. What we do have includes texts which clearly echo language associated with Hippodamos, e.g. *achri tes hodo tesde to astu nenemetai* 'up to this road the *astu* has been assigned' (*IG* i² 893 = i³ 1111), *achri tesde tes hodo teide he Monichias esti nemēsis* 'up to this road in this direction is the assignment of Mounichia' (*IG* i² 894 = i³ 1113). Others mark obviously public areas, the trading area (*emporion*), ferries and roads (*IG* i² 887 = i³ 1101; i² 890 = i³ 1104; two of each). Others use *demosios*, the enigmatic lounges, *lescheon demosion horos* (i² 888 = i³ 1102), a lost text, perhaps of doubtful reading, distinguishing a public mooring from others, || *hormo demosio horos* (i² 889 = i³ 1103), and perhaps five (two not certainly from the Piraeus) defining, surprisingly, a public gateway (i² 891 + *SEG* x 379, xiv 27 = 1097, 1105–8). But land is also so defined. Two texts (i² 892 + *SEG* x 380 = i³ 1109, 1110) proclaim *apo tesde tes hodo to pros tō limenos pan demosion esti* 'from this road on the harbour side everything is public'. It is sufficiently clear that, in the planning of the Piraeus, the designation of public property was of major importance. At a guess, the point of thus designating it in the case of the last area was at least as much a matter of preventing private encroachments as of reserving it for state use.

This little efflorescence of *horoi* of public property appears to have remained unparalleled. Only one *horos* later than the fifth century seems to have used *demosios* at all, for a public road in the Roman period (*IG* ii² 2628).

It should further be noted that, as far as I can see, the area between the road and the harbour at Piraeus is the only piece of public land in Attica not designated by function. No text encourages us to think that the Athenian state ever retained, worked, or leased anything called *ge demosia* (public land). All we find are public buildings, *demosia oikodomemata*, which the council had to supervise (Arist. *Ath. Pol.* 46.2). We know quite a lot about the activity of the *poletai*, the board responsible for state leasing. The language of the *Ath. Pol.* does not absolutely forbid us to suppose that they ever leased any public land or buildings, but there is no evidence in their own documents that they did. The thought did occur to Xenophon that there were possibilities here to adorn the city and increase revenues (*Poroi* 3.12) for the construction of business dwellings and shops, presumably on a rental

basis; Euboulos does not seem to have taken that precise hint,[15] though he did improve the buildings in the trading area (Dinarchus, 1.96). Archaeologically speaking, there are various places and periods where we may suspect commercial intrusion into public buildings, but, as far as I understand current doctrine, purpose-built shops start with the Stoa of Attalos in the second century.

The one case where the *poletai* lease land which is not obviously || sacred seems to take us into a rather different sort of situation. It comes in the document I published in 1959 (this volume, 252–62; *SEG* xviii 15; Schwenk, *Athens in the Age of Alexander* no. 17), a law of the 330s ordering them to lease an area called 'the New Land' (*he Nea*) in two sections, apparently to produce revenue for the Lesser Panathenaia. Nothing actually proves that it is being thought of specifically as public land, and it is clearly something pretty abnormal. Virtually everyone has followed Robert's suggestion[16] that *he Nea* is the newly returned territory of Oropos. I still see some difficulties of detail in reconciling this with the evidence of Hyperides about what happened there, and more still in the light of a new text which apparently records a survey of Oropos.[17] But, accepting Robert for the moment, we must surely regard the problems of a sudden extension of state-territory as very exceptional.[18] There would be an obvious mirror-image of this transaction in what happened when Plataea was destroyed in 427 (Thuc. III.68.3). After an interim period in which Megarian exiles and pro-Theban Plataeans were allowed to live in the city, it was demolished, its materials used for an inn and a temple to Hera, and confiscating (*demosiosantes*) the land, the Spartans leased it for ten years, and Thebans cultivated it. Even without contiguity such circumstances could occur. We could similarly assimilate to annexation the first Athenian solution to the future of Lesbos, also in 427 (Thucydides saw many parallels between the events). The four revolting cities were divided into three thousand lots (*kleroi*). Three hundred of these were reserved for the gods, and the remainder were leased back to the Lesbians, with the rents going to the Athenian cleruchs. It is doubtful whether that solution lasted long, but it is certain that Athens continued to

[15] *Contra*, G. L. Cawkwell, *JHS* 83 (1963), 64.

[16] *Hellenica* xi–xii (1960), 189–203. Langdon (n. 17) has a new suggestion.

[17] M. K. Langdon, *Hesperia* 56 (1987), 47–57.

[18] For some modern confusions between extending a city's territory and extending its property, see A. H. M. Jones, *The Greek City* 359 n. 67.

claim that she had acquired property in the empire, a claim not to be given up until the states concerned joined the Second Athenian Confederacy in 377: 'the *demos* shall give up all the possessions, private or belonging to the Athenian state (*demosia Athenaion*), in the land of those who make the alliance, giving them firm assurances' (Tod 123. || 27–31). Not all of this can have been buildings for Athenian garrisons and governors; some of it was surely agricultural land.

Before returning to other possessions of the Athenian state, we should take some kind of look at public land elsewhere, though I do not pretend to have done very much of the work necessary. My guess is that, in the classical period, Athens may not have been particularly untypical in not having the custom of holding land as state land.[19] Of course the concept exists. In an unpublished text of the early second century,[20] the Thessalian town of Skotoussa sent commissioners round its wall circuit to establish the status of the land adjacent to the walls; among the things which they determined was what land was to be public (*damossos*) and what private (*iddioustikos*). It seems to me that the object of that particular operation was to make sure that certain ground should be kept clear in order not to hamper defence. Some of the evidence takes us into a world where the conditions are to some extent different. Take the case of Zelea in Hellespontine Phrygia, where we have a substantial text (*SIG*^{3} 279), apparently from soon after the departure of the Persians. There is apparently a fair quantity of public lands (*choria demosia*, elsewhere *demosiai geai*) and a strong suspicion that individuals have encroached on them. A board, to be composed of uninvolved persons, is appointed to investigate and fix appropriate prices. For citizens three possibilities are envisaged: (1) they just pay up and keep the land; (2) they claim that they have already bought it or validly acquired it from the city (if that claim fails, they are surcharged 50 per cent); (3) they simply leave, and the board has to sell the vacant land fairly rapidly. There is provision for spending the proceeds of sale on the public temples and other needs of the city, but I do not get the impression that the main motives are financial. Whatever the previous situation had been (I suppose it is most likely that the land concerned had been owned by Persians or their sympathisers), independent

[19] For a different judgement on later periods, see Jones (n. 18), 245–6.

[20] V. Missailidou-Despotidou, 'A Thessalian Inscription and its Topographical Implications'.

Zelea does not want to keep citizen tenants. One category of public land will however remain unaffected, that held by || Phrygians who are paying *phoros* on it; they cannot be turned into owners.

I return to Athens.[21] As military equipment became more specialised the state will have had to acquire more and more which could only be public property; I do not suppose that there were many private catapults. The horses of the cavalry were, however, private, though a sort of insurance location was paid for them.[22] But Athens' principal military property was her navy. Its buildings we have already covered by implication, but the wasting capital tied up in its hulls, equipment, and stores, was enormous. Apart from the private trireme fielded by the elder Alcibiades in 480, the triremes were of course *demosiai*. I doubt if it often occurred to anyone to make the point, but Xenophon (*Poroi* 3.14) does, as an analogy for his proposal to acquire and lease public merchant-ships.[23] He has been reproved for that proposal;[24] was it not enough trouble to keep the navy in repair? There is a possible parallel, which would justify the criticism. When Olbia needed to transport blocks of stone around 230, it had to have recourse to private transport because *ta ploia ta demosia* were in bad condition (*SIG*^{3} 495. 146–151).[25]

I turn now to a very different category of public property. *Demosios*, at Athens and elsewhere, develops into a noun, understanding *doulos* (slave). The public slaves of Athens have hardly been studied more than incidentally since O. Jacob's rather uneven *Les Esclaves publics à Athènes* of 1928, and it may not be all that clear what sort of size of work-force they constituted.

For readers of Aristophanes, the first constituent to come to mind will be the force of Scythian archers. Scholars used to || accept some very high figures for this police-force, running up to 1,200. I am sure that Jacob was right to point out that they had confused the police-archers with general figures given for the size of the force of military archers. He himself was not inclined to go beyond 300 at the outside, and I would guess that even that was too

[21] I slide over various minor objects owned by the state, which were labelled *demosia*. In the earliest of these, a series of bronze weights of about 500, the label *demosia Athenaion* of course reveals as much about its official character as about its ownership. The public seal, the *sphregis demosia*, and the public coin-type, the *demosios charakter*, involve the same kind of usage.

[22] J. H. Kroll, *Hesperia* 46 (1977), 83–140. [23] It is hardly clear what *ta alla demosia* are.

[24] Cawkwell (n. 15), 64.

[25] I do not know why Philippe Gauthier (*Un Commentaire historique des Poroi de Xénophon* 108) thinks these were warships.

high. In any case, it died off; we hear of it last in Aristophanes' *Ecclesiazusae* of 392, and its functions seem to have been performed thereafter by less exotic persons, perhaps simply attached to various magistrates rather than a corps.

Having started with the administration of justice, we should proceed first to the courts.[26] The complexities of the equipment required for their administration were considerable, and the *Ath. Pol.* from time to time refers to individuals who are clearly slaves in its description of the operations; there is indeed a clear reference in Plutarch's *Demosthenes* 5. 2 to the *demosioi* who open the courts. Given that there were several courts (a fact that *Ath. Pol.* tends to lose sight of), we should, I think, be reckoning in terms of a staff well into three figures.

Just as *the demosion* was the gaol, so *the demosios*, sometimes the *demios*, was the public executioner. Considering various indications in Plato's *Phaedo* and Plutarch's *Phocion*, it took a good deal more men than him alone to run the gaol, even with the relatively undeveloped nature of the imprisonment, torture, and execution process in classical Athens; somewhere between ten and twenty does not seem unreasonable.

The *Ath. Pol.* mentions one or two other work-forces. The five road-builders (54.1) have *demosioi* workmen to repair roads. The city commissioners (*astynomoi*) (50.2) have all sorts of functions which might need labour, but their only *demosioi* assistants who happen to be mentioned are those who remove corpses from the roads.[27] It may well be legitimate to suspect that other boards also have them. It seems unlikely, for example, that the superintendents of shrines, who have half a talent a year for repairing shrines (50.1), relied either on working with their own hands or on hired labour.[28] ||

No figures can be suggested for these. For hard figures and some idea of scale, we have to turn to the Eleusis accounts, which are fortunately nice and clear. We notice first that what we are dealing with here are *demosioi*, with no sacred language about them, unlike the *hieroi paides* who turn up in the service of Apollo at Didyma (*Didyma, die Inschriften* 41.60); their employers, the commissioners of Eleusis, are after all state officials. In *IG* ii²

[26] Jacob, *Les Esclaves* 87ff.

[27] Is this because roads are *demosia*? Final responsibility for burial rests with the local demarch (Dem. XLIII.57–8).

[28] I am not clear under whose authority the *andrapoda* are operating who are demolishing the crag and working on the theatre in *IG* ii² 1629.1010–29.

1673, now tentatively dated to 333/2,[29] there were 28 *demosioi* on the strength to be clothed, shod, and fed. But a new fragment shows that 9 of these were stone-transporters, perhaps temporary accretions to the normal establishments. If we leave them out of account, the normal establishment becomes 19, comparable with the 17 well attested for 329/8 (*IG* ii² 1672.5, 42, 71, 117, 142).

To turn to more general administration, a fifth-century inscription from the theatre of Dionysos had long attested the existence of assistants (*hyperetai*) of the council (*IG* i² 879 = i³ 1390), but it was some years after Jacob wrote that actual names started turning up. We now have evidence starting from the middle of the fourth century for the allocation of *hyperetai* to the council on the basis of one per tribe (*Agora* xv 37, 62, 72). It is roughly at the same time (*IG* ii² 120.12 of 353/2) that we get our first *demosios*, one Eukles, named in a public decree and ordered to come and help make a list of the contents of the Chalkotheke. A body of such high-grade *demosioi* had of course existed for some time. The Coinage Law of 375/4 (*SEG* xxvi 72), which begins by ordering the already existing public coin-tester (*dokimastes ho demosios*) to sit in the Agora, goes on to provide for a new one to sit in the Piraeus, to be selected from *the demosioi*, if possible; if not, a new one is to be bought (lines 37–41). Other references to such persons are to be found, in the *Ath. Pol.* (47.5) and in inscriptions, and it seems unlikely that any public transaction took place without the presence of one or two, providing civil service continuity.[30]

I suspect further that actual references to these administrative *demosioi*, either by function or by name, are merely singling out cases where the slave concerned happens to be abnormally || visible. It may be the case that, in the 320s, there was in the dockyards one Opsigonos, important enough to be quoted as *the demosios* (*IG* ii² 1672.197) or even, alongside Dikaiogenes the general, as 'the *demosios* Opsigonos, the one in the dockyards' (*ibid.* 381–2), but I doubt whether the singular has any exclusive meaning. The superintendents of the dockyards had other staff as well, and were able in 357 to lend a *hyperetes* to a trierarch in search of missing naval equipment (Demosthenes xlvii.35) to do odd jobs for him like screaming into the street for citizen witnesses (*ibid.* 36).

[29] K. Clinton, Ἀρχ. Ἐφ. 1971, 112.

[30] For *demosioi* in charge of Athenian weights and measures in the late 2nd cent. BC, see *IG* ii² 1013.

When the new coin-tester for the Piraeus gets going, he is to be paid from the same source as the mint workers. That in itself strengthened the existing presumption, based on a fragment of Andocides about Hyperbolos' father (schol. Ar. *Wasps* 1007), that the workers in the mint were public slaves. I gather that all doubt is now removed by an unpublished law of 354/3 from the Agora (Agora I 7495).

My feeling is that it would be conservative to suggest that the Athenian state owned several hundred slaves in the fourth century, and that there would be nothing particularly surprising if the total ran into four figures. It is against this background that Xenophon's proposals (*Poroi* 4.13ff.) for the city to acquire *demosia* slaves and lease them for work in the silver-mines can be assessed, though, on the face of it, his proposed eventual expansion to a scale of three for every Athenian citizen would involve a very considerable increase in the size of the state's holding.

There can have been in the air some feeling that it was more proper that the city should be the owner of large bodies of slaves. Whether this is connected with, for example, the fact that Spartan helots were not the private property of individuals but were in a sense *demosioi* slaves (Strabo VIII.5.4, p. 365), I do not know. The thought had occurred to some that a city might be better off if all craftsmen were public slaves, but that was hardly practical. In noting that, Aristotle (*Pol.* II.1267B15ff.) does offer the view that at least those who work for the state (*tous ta koina ergazomenous*) should be *demosioi*. That, he says, was the case at Epidamnus, and Diophantos once tried to arrange it at Athens. The passage has not attracted much attention, I think, but this must be the Diophantos, who was || the shadowy coeval of Euboulos. Perhaps such a phenomenon as the increase in the body of *demosioi* at Eleusis in the 330s in order to provide for stone-hauling reflects a feeling of this kind that it was better for the state to own slaves and do a job itself than to privatise and put work out to contract. The argument cannot have been purely economic.[31]

Reviewing the Athenian evidence as a whole, there would seem to be three special factors which may have influenced the growth and nature of public property. First, the tyranny. We can hardly prove that Athens inherited,

[31] It is in the context of financial economy that a Roman governor of the 1st cent. AD recommended Ephesos to replace citizens doing servile jobs by public slaves (*Inschriften von Ephesos* 17.42–4 = 18.13–18), but a social view is also present.

say, the silver mines or the Scythian archers from the tyrants, but both are clear possibilities. Secondly, the empire, creating the need and providing the resources for a greatly enhanced public sector for warfare and administration. Thirdly, democracy. Officials appointed by lot and not particularly wealthy could not be expected to provide the equipment and personnel for many functions which may have been performed in other states by aristocrats, but the jobs still had to be done.

I have not yet been able to think of other possessions of the Athenian state, but Athens certainly did not cover the whole spectrum of possibilities.[32] We cannot be sure that Athens never bought guard-dogs, as they did at Teos (*SEG* xxvi 1306. 19–21), but she never seems to have felt the need for a National Stud and left the winning of international horse-racing prestige to individuals. The main contrast here that we know about is with Argos with its *demosios keles* winning at Olympia in 480 and *demosion tethrippon* in 468 (*P. Oxy.* ii. 222). The public stable at Argos still existed around 420 (Isoc. xvi.1), and I do not suppose that it was a unique institution there. Without going all the way back to the collective victory of the Eleans of Dyspontion in 672, I would suppose that there was || some fairly solid framework of convention behind the circumstances in which the Spartan Lichas entered his chariot at Olympia in 420 as the public property of the Boeotians (Thuc. v.50.4).

This paper has not been concerned with sacred property or the property of organisations below *polis* level. Functionally, we have only considered part of the phenomenon of public property. Although the Athenians drew their distinction between *demosia* and *hiera*, even going to the lengths of charging themselves interest when they borrowed from Athena, I do not think that we can rationally support their attitude. It was they themselves, after all, who decided that Athena was going to make the loan. Similarly, the emphasis which I have laid on the fact that Athens rarely retained land for leasing as public property ceases to be very meaningful when we consider that there was sacred land at Athens which was leased on the instructions of the Assembly by public officials. Admittedly, the proceeds were presumably used for sacred purposes, but, as I have said, sacred purposes are an integral

[32] The public flocks of Miletos, herded by public shepherds, which appear in some books, turn out to be an imaginative expansion by Haussoullier, *Études sur l'histoire de Milet et du Didymeion* 250, of the public wool stolen there by Verres, according to older texts (Cic. *Verr.* 2.1.86); there wasn't even any public wool, that I can see.

part of state expenditure. More general purposes of the state can also be served. Xenophon's suggestion about stimulating economic activity by bringing vacant property within the walls into use may not be documentable in terms of public property, but the efflorescence of mass leases of sacred property now attested for the 340s and 330s[33] can hardly be totally unconnected.

The scale of such non-private property has been the subject of guesses by Andreyev and myself,[34] ranging from 5 to 10 per cent. If this were right, we should not be entitled to apply it beyond Attica. The scale of sacred property in Greek lands will have varied widely. More particularly, it can be expected to rise in states where cult was the most important activity. We have already seen that the Delian and Delphian Amphictyonies were capable of retaining and leasing confiscated property, and we can envisage other situations in which temple property played a more substantial part in the economy; Artemis at Ephesos is an obvious one. At this stage, the difference of scale turns into a genuine difference in the nature || of the society we are looking at, and perhaps this after all is the justification for having tackled public property at Athens on the more confined definition.

Many problems remain. It is by no means clear why Athens was not prepared to retain agricultural land in state ownership. The editors wonder whether there was some recognition of the inefficiency of revenue-raising by public exploitation, but no such inhibition is found in the administration of the silver-mines. Above all, a great deal of further study is needed of the operation of the other types of non-private property, whether belonging to gods or associations, both in the purely Athenian context and in contrast with other states.[35] Again, it is worth considering whether the problem we saw about the status of abandoned property is matched by that of areas which had never been effectively owned at all; much of Greece is forest, mountain, or upland grazing.[36] The question of what constitutes property could hardly

[33] M. B. Walbank, *Hesperia* 52 (1983), 100–35, 177–231.

[34] M. I. Finley (ed.), *Problèmes de la terre en Grèce ancienne* 198–9 = this volume, 275–6.

[35] To take one well-known example, Athena never lent money to states or individuals as Apollo of Delos did (Tod 125), but the local Attic shrine of Nemesis at Rhamnous did lend to individuals (ML 53).

[36] The editors are reminded of the phenomenon of the *eschatiai* which I discussed in Finley (n. 34), 210–12 = this volume, 291–3. We could also consider the frequent appearance in a heavily wooded area of the word *anamphisbetos* in Langdon's new text (above n. 17); Langdon (p. 52) takes it as meaning that its status was not in dispute, but I incline to think that no one was claiming it.

be more sharply raised, and merges into the more difficult question of the growth of a distinction between property owned by the Athenians collectively, implied in stories about mass distribution, and property owned by the state. That touches on the most fundamental questions of the nature of the *polis*.

APPENDIX

Demos and some of its cognates

As readers of Appendix I of Whitehead, *Demes of Attica*, will be aware, the Liddell and Scott article on δῆμος is by no means satisfactory. || Whitehead's main concern is to establish that there is nothing particularly new about Kleisthenes' use of it for his new village communities. With a slight modification, I agree, but I am a bit doubtful about some of the more general issues.

Liddell and Scott start δῆμος as '*district, country, land*' with ample epic justification. As I.2 they sling in, on the side, 'the *people, inhabitants* of such a district', quoting *Il.* III.50, where Hector describes Paris' behaviour as a μέγα πῆμα to his father πόληϊ τε παντί τε δήμωι, and adding two rather arbitrary references, the first to the πᾶς δῆμος which is to build a temple under the πόλις of Eleusis (*Hym. Cer.* 271). But I.2 is evidently regarded as a false start, and we pass to II, 'hence (since the common people lived in the country, the chiefs in the city), the *commons, common people*', starting with the δήμου ἀνήρ in *Il.* II.198, who is certainly contrasted with the category of being a βασιλεύς or an ἔξοχος ἀνήρ ten lines earlier, but continuing with the two sons of Merops, ἀνέρε δήμου ἀρίστω, at *Il.* XI.328, who are not, to my mind, relevant. As Whitehead has shown, II, despite its later importance, is relatively uncommon early. It seems to me that its main distinguishing mark is that it appears in contrast with βασιλεῖς or some higher body. Thus it may already be implied in Hes. *Op.* 260–1, where the δῆμος has to pay for the ἀτασθαλίας of the βασιλεῖς, and in Tyrtaeus, fr. 4 West (the Rhetra fragment), the δημότας ἄνδρας and the δήμου πλῆθος are at least different parts of the body politic from the βασιλεῖς and the γέροντες.

Liddell and Scott's III, 'in a political sense, *the sovereign people, the free people*', is presented as a new start. Whitehead comments on that: 'though as a historico-political development its emergence from usage ⟨II⟩ is plain enough'. I think I

disagree. The earliest instances quoted are from the *Seven Against Thebes*. That is obviously unsatisfactory, even on the narrowest definition of 'political'. The well-known sixth-century text from Chios (ML 8) speaks of δήμο ῥήτρας. The earliest Attic decree (*IG* i³ 1) begins ἔδοξεν τõι δέμοι and I have claimed that *IG* i³ 105.35 τάδε ἔδοξεν ἐλ Λυκείο τõι δ[έμοι τõι Ἀ]θε[να]ίον is a copy of a late-sixth-century text. There is no real difference between this and the seventh-century text from Dreros (ML 2) ἄδ' ἔϝαδε πόλι. As we might well have suspected from *Il.* III.50, πόλις and δῆμος can be interchangeable. I see no discontinuity between that text, *Il.* XI.328, *Hym. Cer.* 271, and the instances we are discussing, except that δῆμος is now found in contexts we should call constitutional. I see no reason to exclude the higher levels of society from these usages of δῆμος and believe them to mean 'the whole people', *populus*. Unlike Whitehead, I derive them from I.2, not from II. In Liddell and Scott terms, there is no firm line to be drawn between their I.2, some of the instances || casually put under II, and their III. Of course, there is continued ambiguity. Students of Solon's poems will be well aware of instances, totally absent from Liddell and Scott, where uncertainty is possible whether δῆμος means *populus* or *plebs*. Andrewes has argued (*The Greek Tyrants* 35–6) that Aristotle had found some early literary passage of constitutional relevance, where he thought that δῆμος should be understood to refer to the hoplites. The differentiation of II from this stream and the development of their I.1 into the village sense do not concern us here. I only observe, in a way I think Whitehead has not quite done, that, although there were δῆμοι in Attica before Kleisthenes in a development of I.1, what Kleisthenes did was to create, for example, a δῆμος τῶν Ἀχαρνέων in a sense indistinguishable from the δῆμος τῶν Ἀθηναίων classifiable as III; this is of course ground covered in Osborne's paper (Murray and Price [eds.], *The Greek City*, ch. II).

From δῆμος I pass to the adjectives. The linguists (I am relying on Chantraine) tell us that both δημόσιος and δημοτικός are not derived from δῆμος but from δημότης, but fortunately they concede that δημόσιος always behaves functionally as if it were derived from δῆμος, so we need not worry ourselves about that. We can also put δημοτικός on one side. Despite some nineteenth-century scholars who tried to turn Nikias into an oligarch on the strength of the assertion (Xen. *Hell.* II.3.39) that neither his son nor he ever did anything δημοτικόν, we are surely now all agreed that δημοτικός is specialised, in Athens, to δῆμος II *plebs*; the only place known to me where it has any official meaning is Olbia, where there is a δημοτικὸν δικαστήριον, supposed to be contrasted with one for foreigners (Tod 195.17).

The main paper's primary concern is with δημόσιος. I hope I have shown that δῆμος *populus* is of sufficient antiquity for δημόσιος to be related to it, that the use of the word need not be programmatic, that, when it starts appearing, there need not be any suggestion of the existence of institutions we might call democracy. Liddell

and Scott is an imperfect guide on δημόσιος as well, omitting the four earliest appearances of it known to me. Two of these are very general, hardly differing very much from Paris's behaviour being a μέγα πῆμα to the δῆμος. Solon fr. 4 talks about the bad state of Athens in general ταῦτα μὲν ἐν δήμωι στρέφεται κακά (23), and passes to how it affects individuals οὕτω δημόσιον κακὸν ἔρχεται οἰκαδε ἑκάστωι (26). δημόσιον κακὸν also appears in the late-seventh-century epitaph ML 4, where it seems to describe the loss to the Corcyrean people of its drowned *proxenos* (it also comes in Theognis 50). That, of course, is the text where δᾶμος itself appears three times, once in the phrase πρόξενϝος δάμου φίλος, twice as the body which made the tomb, just as the Eleusinian δῆμος was to build the temple to Demeter; I stress again || that there is no implication of democracy. Rather different is the appearance in the Chian text we have already alluded to; here the various appearances of δῆμος are matched, as is well-known, by the reference to the βολὴ ἡ δημοσίη, but to relate that to what I have said about δῆμος has no bearing on the main paper. It is the fourth reference, Solon fr. 4.12–13, from which the treatment there starts.

10

Cleisthenes and Attica

There has been much recent work on Cleisthenes.[1] The justification for yet another article lies, I hope, in its different approach, from the land of Attica itself and the framework which Cleisthenes gave it. I hope also that this difference of approach will excuse what may seem a somewhat cavalier attitude to my immediate predecessors, whose arguments will seldom appear, although I have read them with attention and profit. The argument of this article proceeds largely from survivals, and leads to the paradox that we can understand Cleisthenes' work best in the places where he failed. These failures, however, give us clearer light not only on his aims, but on the woven texture of tradition and innovation which is formed by the life of classical Athens.

I REGIONAL PARTIES IN SIXTH-CENTURY ATHENS

Let us begin by sketching our evidence for regional divisions in Athenian politics before Cleisthenes' reforms. The parties of the 560s have regional names, acquired, we are told,[2] from the places where they farmed. We should emphasise that parties may include members from outside the original area

* Published in *Historia* 12 (1963), 22–40. Much has been published since then on Cleisthenes' organisation: in addition to the books reviewed in this volume notice especially J. S. Traill, *Demos and Trittys*; G. R. Stanton, *Chiron* 24 (1994), 161–207.

[1] The two important items of the last fifteen years which do not appear in my notes are Larsen, *Representative Government* ch. 1, and Bradeen, *TAPA* 86 (1955), 22–30. The second starts from presuppositions which are here rejected, the first I cannot accept, largely because I believe that the bouleutic oath is more likely to have formalised than to have altered the situation which existed before it. This paper has been read by A. Andrewes, W. G. Forrest and A. E. Raubitschek, and much improved by them. They are not responsible for the blemishes which remain, and would, I think, differ from me on several points. (See also Hopper, *BSA* 56 (1961), 189–219.)

[2] *Ath. Pol.* 13.5.

of the nucleus. When Peisistratus waited at Marathon in 546, supporters came to him from Athens itself as well as from the country-villages.[3] This should also act as a warning against any easy assignment of single economic or even constitutional motives to a party. The party may be a complex of driving-forces held together by its leader; such a complex is in fact sketched for Peisistratus' party by Aristotle.[4] However, even with these qualifications in mind, it is legitimate to look for the nuclei.

The *pedieis* are relatively straightforward. None will doubt that their nucleus is in the plain of Athens itself, not indeed the whole of it, for we shall || see reason to exclude the area south and south-east of the city, but at least the more substantial portion to the north and west. Support for this comes from the name of their leader, Lycurgus.[5] The chances are high that he, like the later prominent men of the name, came from Boutadai, a little to the west of the city.[6] No other family can be confidently associated with this party, but one may be tempted to go back to Myron of Phlya, prosecutor of the Alcmeonids for their action against the Cylonian conspiracy.[7] He will have come from the north-east of the *pedion*.

The *paralioi* may best be located by looking for Alcmeonid land. Alcmeonids are found in three demes, Alopeke,[8] Agryle,[9] and Xypete,[10] all to the south and south-east of the city.[11] They farm the land which goes down towards the coast, and will thus have got their name. Two other families are to be associated. The principal family of the Kerykes, headed in this period by Kallias son of Phainippos, is attested as notably anti-tyrant by Herodotus.[12] After many wanderings in modern scholarship, it is now firmly settled in Alopeke.[13] Another Alopeke family, later allied with this one by marriage,[14] is the house which produced Aristides,[15] who, we are

[3] Her. 1.62.1.

[4] *Ath. Pol.* 13.4–5. My idea of the 'Parties' does not differ substantially from that formulated by Sealey, *Historia* 9 (1960), 163–5.

[5] Her. 1.59.3. [6] *P.A.* 9249–9251. [7] Plut. *Sol.* 12.4.

[8] Μεγακλῆς Ἱπποκράτους (*Ath. Pol.* 22.5 and numerous ostraka) and others.

[9] Λεωβώτης Ἀλκμέωνος (Plut. *Them.* 23.1); cf. also Ἀλκμεωνίδης Ἐρεχθηίδος (Tod 26.135).

[10] Καλλίξενος Ἀριστωνύμου (*Hesperia* 19 (1950), 376ff.).

[11] Sealey, *Historia* 9 (1960), 163 n. 41, adds Leukonoe from *IG* i² 368.3–4. This must remain uncertain, since no certain Alcmeonid is called Megakleides. If he were accepted, we would have Alcmeonid land north of the city.

[12] VI.121. [13] *Hesperia* 5 (1936), 410, *BSA* 50 (1955), 13–14, *JHS* 81 (1961), 120.

[14] Plut. *Arist.* 25.4. If Aristides had been himself a Keryx, we would have been told.

[15] *Ibid.* 1.1.

told,[16] was in early life a *hetairos* of Cleisthenes. Alopeke is reasonably to be identified as the headquarters of the party, and this finds an echo in a partisan drinking-song which will go back to this period[17] οὐκ ἔστιν ἀλωπεκίζειν, οὐδ' ἀμφοτέροισι γίγνεσθαι φίλον, where ἀλωπεκίζειν refers as much to the political shifts of the men of Alopeke and their leader Megakles as to the cunning of the fox.

Both of these parties will have had their supporters in the city. Alcibiades of Skambonidai and Leogoras of Kydathenaion, head of the other main branch of the Kerykes, could claim opposition to Peisistratus later.[18]

The *Hyperakrioi* are, on the face of it, the men beyond the hills, outside the Parnes-Pentelikon-Hymettos ring.[19] *Diakrioi*, the main alternative form of their name, will come from the Diakria, which is said, on the most plausible || emendation of Hesychius,[20] to run from Parnes to Brauron. It is at Brauron itself that we can safely place their leader Peisistratus.[21] Marathon, further north, is well within their area, and it is tempting to think that the tyrant's party landed there in 546 and 490 because they could be sure of local support. The direct arguments for Peisistratid supporters in this area are not however very strong. Wilamowitz[22] thought that the Aristion who moved the decree for Peisistratus' bodyguard[23] was the Aristion commemorated in the well-known relief from Velidaneza, who would thus have come from half-way between Brauron and Marathon, but the date of the relief is too late[24] and the name not uncommon. More hopeful supporters might be found in the two Marathonians found on ostraka of the 480s. Of these, Boutalion

[16] *Ibid.* 2.1. His father Lysimachos may be the *tamias* of *IG* i² 393 = i³ 510 which presumably dates from a period of Peisistratid exile, since it has an Andocides (see n. 18). But J. K. Davies points out to me that this Lysimachos could also be a Boutad, ancestor of the family analysed in *BSA* 50 (1955), 7, which would make as good political sense.

[17] Ar. *Wasps* 1240–1. [18] Isoc. XVI.26; And. 1.106, II.26.

[19] So Wade-Gery, *Essays in Greek History* 167 n. 2.

[20] *s.v.* Διακρεῖς . . . καὶ ἡ χώρα Διακρία, ἡ ἀπὸ Πάρνηθος εἰς βαλυλῶνος. Ure, *Origin of Tyranny*, 312, suggested ἕως Αὐλωνος which has reached Latte's *apparatus*, but Aulon is of no importance and forms no kind of landmark to define a boundary. Clear evidence for Brauron in this context comes from Bekker, *Anecd.* i. 242 Διάκρια· τόπος Ἀττικῆς ὑπὸ Βραυρῶνα Ἐλευσίνιον Δήμητρος καὶ Φερεφάττης ἱερόν. I would put a full stop after Βραυρῶνα, and regard the last five words as a totally different gloss, wrongly incorporated.

[21] Plato, *Hipparchus* 228B, Plut. *Sol.* 10.3.

[22] *Aristoteles und Athen* i.261; cf. Burn, *Lyric Age of Greece* 305. [23] *Ath. Pol.* 14.1.

[24] See most recently Richter, *The Archaic Gravestones of Attica* 47. She puts it *c.* 510.

is a serious possibility for the missing name of the pro-tyrant ostracised in 485.[25] The other, Habron son of Patrokles,[26] I was once inclined to think identical with a Habron, archon under Hippias in 518/17,[27] possibly son of the Patrokles who dedicated the mid-sixth-century altar of Athena Nike,[28] but Lobel[29] has now made it highly unlikely that there was such an archon. The case for Peisistratid supporters in Marathon must remain largely one of probability.

Hyperakrioi is, I take it, a wider term than *Diakrioi*, and will accommodate, besides these north-eastern hills, the East Attic plain which we now call the Mesogeia. The population of this plain must already have been substantial in the time of Cleisthenes, for the descendants of the demesmen of 507 provided over an eighth of the fourth-century *boule*. It is, however, extremely short of early families of importance. The one sixth-century figure is Socrates of Paiania, father of the Phye who played a part in Peisistratus' first return and married his son Hipparchus.[30] Paiania lies directly on the only route from Brauron to Athens. The Mesogeia has been described as 'the plain dominated by Brauron and Marathon.'[31] This is slightly misleading. Marathon is very much tucked away behind Pentelikon, and a sharp uphill walk is needed to take one out of the marshy valley of Brauron itself to the 120-metre level of the main Mesogeia. Nevertheless it is true that, in the absence of powerful || families in the plain itself, Marathon and Brauron, both with important cults to serve as a focus, could exercise influence on the plain, which would tend to look east to them before it looked west to Athens.

Two more families of importance remain for consideration. Since Wilamowitz[32] it has been normal to associate the Philaids with Brauron and the *Hyperakrioi*.[33] The case for this has been substantially political, and the only topographical argument is that Philaios is said to have settled at Brauron, which was called Philaidai in the Cleisthenic deme-system.[34] However, it can hardly be true that Philaios bound his descendants to the site forever. On this argument, it would be surprising that Themistocles came from Phrearrhioi and not from the Lykomid centre at Phlya, and in fact the

[25] *Ath. Pol.* 22.6. *Hesperia* 21 (1952), 8 with n. 15.

[26] *Hesperia* 6 (1937), 155–6, Supp. 8 (1949), 409. [27] Cadoux, *JHS* 68 (1948), 112.

[28] Raubitschek, *Dedications from the Athenian Akropolis* no. 329.

[29] On *P. Oxy.* xxvi.2438. [30] Kleidemos 323F15. [31] Sealey, *Historia* 9 (1960), 165.

[32] *Aristoteles und Athen* ii. 73–4.

[33] Wade-Gery, Hignett, Hammond, Sealey, Burn, to name only a few.

[34] Plut. *Sol.* 10.3.

ancestors of Epicurus, the only man described as a Philaid by an ancient source, lived at Gargettos, well away from Brauron, in 507.[35] There has never been any doubt that the Kimonids, who succeeded to the position of Miltiades son of Kypselos, lived elsewhere. Their family estates were at Lakiadai,[36] west of Athens on the Sacred Way, and Kimon and doubtless his father bore that demotic.[37] Their family tombs were at Koile, just outside the city to the south-west.[38] They may indeed have been a different family, although it has recently been observed[39] that it would make better sense of Miltiades' adoption of Stesagoras II if Stesagoras I had also been a Philaid. A neglected Pindaric scholion even positively asserts the descent of Kimon from Aias.[40] A stronger positive reason for detaching the Philaids from Brauron may be found in the story of Miltiades' meeting with the Dolonkoi.[41] The Dolonkoi leave Delphi, go through Phokis and Boeotia, receiving no invitations, καὶ ἐκτρέπονται ἐπ' Ἀθηνέων, that is, they turn along the Sacred Way through Eleutherai and Eleusis. Miltiades, sitting by his front door, sees them passing and shouts his invitation, certainly not from Brauron, but rather, we may confidently assert, from Lakiadai. Territorially, at any rate, the case for supposing the Philaids *pedieis* seems strongest.

Cleisthenes' opponent, Isagoras, presents a more difficult problem. Two recent scholars[42] have made him a Philaid, largely on the strength of his father's name, Teisandros. The name appears in other families too, and the || case is hardly strengthened by a mysterious brother of Miltiades II, with a name ending in -agoras, who appears in 489.[43] Herodotus[44] says firmly that he does not know Isagoras' family, but that his συγγενέες sacrifice to Ζεὺς Κάριος. It is hard to believe that he discovered the family cults and not the Philaid blood, and even harder to believe him a deliberate liar. The best approach is probably through the family cult. Ζεῦς Κάριος has been thought

[35] Diogenes Laertius, x.1. [36] *Ath. Pol.* 27.3.

[37] Plut. *Cim.* 10.2; cf. *IG* i² 295.8 = i³ 364.8.

[38] Her. vi.103.3, Marcellinus, *Vit. Thuc.* 55. [39] Burn, *The Lyric Age of Greece* 311.

[40] On *Nemean* ii. 19, from Didymus, but I have an uneasy feeling that Didymus may have got muddled and thought that the Pherecydes genealogy quoted from him by Marcellinus, *Vit. Thuc.* 3 proved the Aiantic descent of the younger branch, which it does not. Doxopater *ad* Aphthon. p. 439, 3 Walz also goes back to this (cf. Schmidt, *Didymi Fragmenta*, 324ff., who misses the point about the relationship of the two branches.)

[41] Her. vi.34–5. [42] Sealey, *Historia* 9 (1960), 172; Hammond, *CQ* n.s. 6 (1956), 127–8.

[43] Nepos, *Milt.* 7.5. [44] v.66.1.

to be a sneer at oriental origins by many from Plutarch[45] onwards. But he seems to be at home in Ikaria in the Pentelikon area, where as simple Karios he seems to be the basic deity, beside whom Dionysos is an importation.[46] Here too we may place the headquarters of Isagoras' *genos*, though he himself may have moved elsewhere.

II CLEISTHENES' REFORMS

A The deme-names

Obviously, there were centres of population before Cleisthenes. Cleisthenes gave them corporate existence by making *demotai* and *demarchoi*.[47] Aristotle[48] notes that not all places had names and Cleisthenes gave them names. The interesting names are those ἀπὸ τῶν κτισάντων, the patronymic deme-names. Some of these, for example, Boutadai and Paionidai, are certainly also the names of *gene*. It is not stated and there is no probability that membership of such demes was confined to members of these *gene*. It is obvious that it would be distinctly weakening to a kinship-organisation if the same name were given to a state unit of more open composition. One clear instance shows that this blow was felt. By the fourth century[49] the *genos* of the Boutadai, which held two of the most important Athenian priesthoods, has renamed itself the Eteoboutadai, the real descendants of Boutes.[50] If the blow was felt, it may well have been intended; the assumption that it was is strengthened when we recall the case for supposing that the Boutadai had headed the *Pedieis*. This may not be the only case, and observation seems to show that there is a concentration of patronymic names in the likely area of the *Pedieis*.

[45] *Her. Mal.* 860E.

[46] *IG* i² 186 = i³ 253. This is at least the present state of our evidence, but Forrest rightly warns me that it is only in line 6 that the deity is unequivocally Κάριος and not Ἰκάριος, and that there may be an error in the reading here. If there is such an error, all reason to connect Isagoras with Pentelikon of course vanishes, and I would withdraw what is said below about the political motive for Cleisthenic reorganisation on Pentelikon, but not the reorganisation itself. [In *IG* i³ Lewis reads Ἰκάριος even in line 6. Rhodes wonders if we should read Διὶ ⟨Ἰ⟩καρίῳ in Her. v.66.1.]

[47] *Ath. Pol.* 21.4–5. [48] *Ibid.* [49] Dem. XXI.182 is perhaps the earliest reference.

[50] Cf. *IG* ii² 3474.3 Βουταδέων ἐτύμων ἐξ αἵ[ματος] and Toepffer, *Attische Genealogie* 117. There is an interesting parallel in the *genos* of the Salaminioi, which renamed itself Archaiosalaminioi *c.* 300 BC (Daux, *REG* 54 (1941), 220–2).

The game could perhaps be played both ways. That Brauron was a place of importance is clear, but its name was a word of power, the epithet of Artemis || Brauronia, and power to the wrong party. Brauron is not the name of a Cleisthenic deme. Instead the deme is called Philaidai, an annoyance to the Philaids in itself and perhaps an attempt to foster such cult of Philaios as there was away from the Philaid centre at Lakiadai.

Territorially the country-demes would have been easy to arrange. If there was gerrymandering at this level, we cannot recover its details. The real problem will have come in the city, and here we are still almost without evidence to determine the principles on which the deme-system was arranged.[51]

B The trittyes

It will be helpful at this point to consider the present state of our evidence on the names of the trittyes and their distribution on the map of Attica. To save space and to avoid *parti-pris*, I shall almost always follow the views of Kirsten,[52] with the single qualification that I cannot follow his belief that it is nearly always possible to draw a neat line on the map round the territory of any individual trittys.

Erechtheis (I). The names of the trittyes are unknown. The city-trittys included no true city-deme, and was mostly taken from the territory of the old Paralia, without going down to the coast. Its principal demes were Euonymon and Agryle, the latter having Alcmeonid land. The coast-trittys was in South Attica, south-east of Hymettos, formed mainly of the demes of Lamptra and Anagyrous. The inland-trittys was in the far north-east of the Attic plain, with Kephisia its most important element. Whether by accident or design, the Boutadai, who held the hereditary priesthood of Erechtheus, were not included in this tribe, which had a priest of Erechtheus appointed by lot.[53]

Aigeis (II). We have the name of one trittys, that of the inland, Epakreis.[54] The word has some history. Strabo[55] gives Epakria as one of the twelve

[51] Young (*Hesperia* 20 (1951), 140–3) produces some evidence to show that main roads acted as boundaries.

[52] The best map is that in *Westermanns Atlas zur Weltgeschichte*, p. 13, the best survey of the position in *Atti del terzo congresso internazionale di epigrafia greca e latina* 151–71, with Tav. XXVI. The evidence is scattered through Philippson, *Die griechischen Landschaften* i.3, and must be traced through the index of sites there, pp. 1065–8.

[53] *IG* ii² 1146.4. [54] *IG* ii² 2490.8, 1172.30. [55] IX.1.20, p. 397.

cities founded by Kekrops. Plutarch[56] once uses Epakreis as the name of Peisistratus' party, probably by simple confusion. An Epakria distinct from the Cleisthenic trittys is also attested by Philochorus.[57] The city-trittys included || Kollytos, which is actually in the city, south-west of the Agora, but it also ran far enough to the north-west to take in Kolonos Hippios and to the north-east to take in Ankyle. It seems unlikely that it was fully contiguous.[58] The coast-trittys was on the east coast, from Brauron north to Myrrhinoutte. The inland-trittys was oddly shaped, lying mostly on Pentelikon, with a long tongue south into the Mesogeia plain to include Erchia. These two trittyes touch, and it is perhaps even doubtful to which trittys Ionidai and Teithras should be assigned.

Pandionis (III). Here we have three trittys-names, known and straightforward. The city was Kydathenaion,[59] the coast Myrrhinous,[60] the inland Paiania.[61] The city-trittys included the Akropolis and therefore the shrine of Pandion, and was formed only of the deme Kydathenaion. The coast-trittys was a powerful block south of Brauron, taking in part of the south-east of the Mesogeia plain. It seems to have touched the inland-trittys at an angle. This trittys was composed of the north of the Mesogeia, with the exception of Erchia. The strange feature of this tribe, to which we shall return, is its inclusion of Probalinthos, which seems likely to have been at Xylokerisa, on the coast immediately south of Marathon and separated from the rest of the coast-trittys by the whole length of the coast-trittys of Aigeis.

Leontis (IV). We have the names of the city-trittys Skambonidai and the coast-trittys Phrearrhioi; the inland-trittys, six letters in the genitive, evades identification.[62] The city-trittys has Skambonidai in the city itself and some of the plain to the north. Part of the northern section of the city-trittys of

[56] *Amat.* 763D.

[57] 328F206. This passage which puts the deme Semachidai, which belongs to the tribe Antiochis, in the Epakria raises an unsolved problem. The only direct evidence bearing on the site of Semachidai is the mention of a Semacheion near Laureion in a mining-lease (*IG* ii² 1582.54). If this is a shrine and not a mine, does it place the deme in South Attica? Trittyes of Antiochis lie both near Laureion and on Pentelikon, and are no help. This is the only piece of evidence for the Southern Diakria envisaged by Ure, *Origin of Tyranny*, and I would doubt whether it was ever really legitimate to argue from Epakria to Diakria.

[58] See *BSA* 50 (1955), 16–17. [59] *IG* ii² 1748.14. [60] *IG* i² 898 = i³ 1127.

[61] *IG* i² 898, ii² 1748. [62] *Hesperia* 9 (1940), 54, 30 (1961), 265.

Aigeis seems to intervene. More oddly still, Halimous, well down the coast to the south-east, is completely separate from it. The coast-trittys has the southern end of the east coast down to Sounion, but the territory of Thorikos is a bite taken out of it, which effectively divided it into two. The inland-trittys has the eastern slopes of Parnes and a portion of the north-west Attic plain. As will be seen, it is difficult to make Hekale, on Pentelikon, join it.

Akamantis (v). Here we begin to run into difficulties, for we have four trittys-names. Cholargos was the name of the city-trittys *c.* 450,[63] but it is difficult to see how *IG* i² 883 = i³ 1118 (*c.* 420) can be restored as anything but [Κερ]αμέων.[64] The coast-trittys was Thorikos,[65] the inland-trittys Sphettos.[66] On the face of it, the city-trittys was renamed between 450 and 420.[67] These two names reflect its mixed composition. Kerameis was certainly at least partly in the city; the rest of the trittys was a part of the plain including Cholargos. Unless one draws one's lines in a very arbitrary manner, the trittys was certainly split. The coast-trittys only contained the deme of Thorikos. The inland-trittys had the whole of the south Mesogeia, except Myrrhinous. ||

Oineis (vi). Here we have two certain trittys-names, Lakiadai[68] and Thriasioi,[69] presumably the city and coast trittyes. A third appears on two stones which appear to confirm each other in the restorations Πε̣[δ]ιέον and [Πε]διέ̣[ον].[70] This was held by Wade-Gery to be the inland-trittys, but he felt the difficulty of this view, since the inland-trittys was composed entirely of the deme of Acharnai and Thucydides[71] seems to draw a distinction between Acharnai and the *pedion*. One cannot exclude the possibility suggested to me by Mr Forrest, that this is a case analogous to Philaidai, a stroke of malice on Cleisthenes' part, putting the trittys Pedieis where there were manifestly no *pedieis* in the party-sense. However, the discovery of a name-change in Akamantis opens another possibility here, for of the two stones which give the evidence for Pedieis, one is certainly,[72] the other possibly, earlier than the stone which gives Lakiadai. A solution which makes Pedieis an earlier name for Lakiadai seems more attractive. For this city-trittys will be the heart of *pedieis* country, the plain west of Athens towards Korydallos, reaching Athens along the Sacred Way through Lakiadai and

[63] *IG* i² 900 = i³ 1131. [64] See Meritt, *Hesperia* 9 (1940), 53–4. [65] *SEG* x 371.
[66] *SEG* x 370 [67] So Raubitschek, *AJA* 60 (1956), 281. [68] *IG* i² 884 = i³ 1120.
[69] *IG* i² 899 = i³ 1128.
[70] Wade-Gery *Mélanges Glotz*, ii. 884–6; Meritt, *Hesperia* 9 (1940), 55, 30 (1961), 265.
[71] 11.20.1. [72] *IG* i² 899 has three-bar sigma.

Boutadai. The coast-trittys is only partly coast. It includes the eastern part of the Thriasian plain, but reaches right up on to Parnes to include Phyle. The inland-trittys, as we have seen, is only composed of the deme of Acharnai.

Kekropis (VII). The trittys-names are unknown. The city-trittys was composed of Xypete and Melite, which cannot be made to join.[73] The coast-trittys was south of Hymettos, and mostly the territory of Aixone. The inland-trittys is geographically uneven, with parts of the north-east Attic plain and south-west Pentelikon.

Hippothontis (VIII). One inscription[74] gives us two trittys-names, Piraeus for the city, Eleusis for the coast. Another,[75] much earlier, gives us a third name, a long one, beginning with Τε or Ζε. Wade-Gery, to whom we owe the restoration,[76] assigned this automatically to the inland-trittys. But no suitable restoration is available, and the possibility of a change of name allows the chance that this may be another name for the city or coast trittys. A new suggestion is possible, and the argument may lead us to one. The city-trittys was Piraeus with the land west along the coast and Koile in the city itself. It may be possible to join these elements, but it hardly seems necessary. The coast-trittys is Eleusis and the west of the Thriasian plain, but it goes up on to Parnes to include Oinoe. The inland-trittys is the extreme east of Parnes, Decelea, Sphendale and Oion, very widely spread in an area of thin population. || The tribal centre, the Hippothontion, was, abnormally, not in Athens, but in Eleusis.[77]

Aiantis (IX). Here we have one name, for the coast-trittys, Tetrapoleis.[78] The city-trittys was composed of Phaleron alone. The coast-trittys will be discussed later. The inland-trittys was in the hills east of Parnes, mostly the territory of Aphidna. The tribal centre was not in the tribe at all, but at the Eurysakeion in Melite of Kekropis.[79]

Antiochis (X). Here we have two trittys-names, Alopeke for the city,[80] Pallene for the inland;[81] Anaphlystos has been plausibly suggested for the coast. The city-trittys was composed of Alopeke alone. The coast-trittys lay in south Attica, running inland from the west coast of Cape Sounion. The

[73] *BSA* 50 (1955), 17. My observations on the location of Melite are, I am told, confirmed by an unpublished inscription from the shrine of Artemis Aristoboule. [Published as AΔ 19 (1964), μελ. 31–3 no. 1 = *SEG* xxii 116.]

[74] *IG* i² 897 = i³ 1129. [75] *IG* i² 901 = i³ 1130. [76] *Mélanges Glotz* ii. 886–7.

[77] *IG* ii² 1149, 1153. [78] *IG* i² 900 = i³ 1131. [79] *Hesperia* 7 (1938), 18.

[80] *Hesperia* 30 (1961), 264. [81] *SEG* x 374.

oddly-shaped inland-trittys covers the Pentelikon-Hymettos gap; its centre was Pallene, which blocks that gap.

In this survey we have on the whole assumed that our evidence, which is mainly fourth-century, is valid for Cleisthenes' reform. We should note that not much of our evidence for the tribal affiliation of demes is earlier than 450, although some of our evidence for the affiliation of trittyes is earlier. We have seen some reason to believe that some trittyes were renamed between 450 and 420, but I do not think that this should deter us from employing the only available working hypothesis, that there was no major reorganisation. Such a reorganisation should not in any case be inferred from the manuscript reading of Her. v.69.2, which seems to assert that there were only one hundred demes in the time of Cleisthenes. It is indeed theoretically possible that splitting will have increased the number of demes, but the reverse process will have had to apply to Aiantis, which never had more than six demes.[82]

C *The anomalies*

It should now be clear that Cleisthenes did not just draw lines on the map. Such lines are difficult to draw, and break down notably in the city. Why the city is organised as it is, we cannot tell, and our questions will produce few profitable answers. Our best line of approach is through the anomalies outside the city, demes which appear to be enclaves, detached from their trittyes. If we can gain some insight into the reasons for these anomalies, we may find that we have some light on the reform as a whole. There seem to be three notable enclaves worth discussion, Probalinthos, Hekale, and Halimous.

For Probalinthos the explanation seems clear, and Kirsten[83] has already in || part given it. One of the cities of Kekrops[84] was the Tetrapolis, which continued to exist as a separate territorial cult-organisation, sending independent embassies to Delphi and Delos, down to the first century BC.[85] It was composed of the four units, Marathon, Oinoe, Trikorynthos and Probalinthos. These demes will have been fairly close together, and a suitable

[82] I should however draw attention to Raubitschek's arguments (*AJA* 60 (1956), 281) for a change in or regularisation of the official order of the tribes in the middle of the fifth century (I believe *IG* i² 943 = i³ 1162 to be *c.* 447 rather than *c.* 440).

[83] *Atti* (see n. 52), 162. [84] Strabo IX.1.20, p. 397. [85] Boëthius, *Die Pythais* 38, 107.

site for Probalinthos has been found at Xylokerisa, confirmed by two gravestones[86] and a dedication.[87] The antiquity and deep roots of this organisation are confirmed by the list of its sacrifices.[88] A body which sacrifices to Ἀθηναία Ἑλλωτίς, Ζεὺς Ἀνθαλής, Χλόη παρὰ τὰ Μειδύλου, Γάλιος, the Νεανίας and a succession of nameless heroes and heroines is not an artificial construction. But the unit formed by these four demes will not only have been territorially compact, but also Peisistratid territory. Efficiency will demand that it has to be some kind of administrative unit. It can even be called the trittys of the Tetrapoleis. What could be done, however, and was done, was to detach from it the prosperous deme of Probalinthos (providing five *bouleutai* in the fourth century)[89] and to attach Rhamnous, which had totally different cults and doubtless different traditions. Probalinthos is detached, but it is attached, not to the contiguous coast-trittys of Aigeis, but to the more distant coast-trittys of Pandionis. The only explanation can be that there is little point in detaching it from Marathon in order to attach it to the trittys which contains Brauron. In its detachment from both, it becomes an enclave, separating the two centres of Peisistratid influence.

Here, on a larger scale, we see the same mind at work as we saw with the Boutadai. The traditional unit remains and keeps its name, but by its side we have a new unit, imposed from outside, with the same name, but of different composition. Even this may not have been felt to be enough, and there is an indication that steps may have been taken to improve the political situation. For, by shortly after 490, it is possible to appoint at Marathon to supervise a festival thirty men ἐκ τῶν ἐπιδήμων, representing each of the ten tribes.[90] The ambiguity of the word allows the possibilities either that these are visitors for the festival or that they are new settlers, perhaps on confiscated property, who have come to Marathon since the Cleisthenic reforms.

The second case is Hekale. Its site is a problem. It is somewhere near the Tetrapolis on the road from Athens to Marathon,[91] which probably puts it somewhere on Pentelikon, and certainly well away from any part of Leontis. It has been proposed[92] to put it at Kukunari on the north side of Pentelikon, where in fact the sacrificial calendar of the Tetrapolis was found. There

[86] *IG* ii² 7292, 7304. [87] *IG* ii² 7296.

[88] *IG* ii² 1358 (some revisions in *AM* 67 (1942), 12–13). [89] e.g. *IG* ii² 1700, 1751.

[90] *SEG* x 2.19–22. [91] Plut. *Thes.* 14, Callimachus fr. 230.

[92] Milchhoefer, *Untersuchungen über die Demenordnung des Kleisthenes* 21–2, Richardson, *AJA* 10 (1906), 219.

was || a Hekalesia at Hekale, celebrated by οἱ περὶξ δῆμοι,[93] and since the Tetrapoleis are in any case so far inland, scholars have wished to equate the groups. But Hekale is not part of the Tetrapolis, and although Theseus stayed there on his way to fight the Marathonian bull, it is not stated that the Tetrapoleis took part in the Hekalesia. Nor do Hekale or Ζεὺς Ἑκαλήσιος appear on the Tetrapolis-calendar, which is admittedly incomplete. It is probably better to see in Kukunari merely a cult-spot on a hill, and I see no real reason to deny that Hekale is at or near the present village of the name, south-west of Kukunari, but still in the north-north-west of Pentelikon. This is still far from Leontis, and the reason for detaching it from the nearby inland-trittyes of Aigeis, Kekropis and Antiochis must be its importance as a cult centre. If we knew which οἱ περὶξ δῆμοι were who celebrated the Hekalesia, we would find them in these trittyes. The suspicion that deliberate interference has been at work in drawing the trittys-boundaries in this part of the world is strengthened by a consideration of the shapes of these trittyes, all distinctly malformed. The political motive may well lie in the position of Isagoras. We have already seen grounds for placing him in Ikaria. There, as at Boutadai, a non-Cleisthenic organisation survived side by side with the deme.[94] The Cleisthenic trittys to which it belonged retained the name Epakreis. We do not know the home or domain of Ζεὺς Ἐπάκριος,[95] but here again we may suspect that territory is removed from it[96] and that non-Epakria territory, Erchia, is brought in from the plain below.

The third anomaly, Halimous, has been explained by Kirsten[97] as belonging not to the city-trittys of Leontis, but to the inland-trittys. The deme, it is said, is composed of Alcmeonid supporters from near Leipsydrion,[98] who moved after 507 to Alcmeonid land, retaining their old trittys-affiliation.

[93] Plut. *Thes.* 14.2.

[94] *IG* i² 187. 3 = i³ 254. 3, ii² 1178.4–5. If I had to make a firm guess, I would say that the Ἰκαριεῖς were a phratry, standing to ὁ δῆμος τῶν Ἰκαριέων as the Δεκελεεῖς stood in relation to the Cleisthenic deme of that name (Wade-Gery, *Essays in Greek History* 133–4). But there are perhaps other possibilities.

[95] *Et. Magn.* 352.49; perhaps *IG* ii² 1294.

[96] If Semachidai is in this area, this would be a certain case. See n. 57.

[97] In Philippson, i. 984 and *Atti* (see n. 52), 161.

[98] The case for supposing that Leipsydrion was in Alcmeonid land is in itself not cogent. It rests on Herodotus' statement (v.62.2) that Leipsydrion was ὑπὲρ Παιονίης and the story in Pausanias II.18.8 that the Alcmeonidai and Paionidai came from Pylos together.

However, the arrangement of a prytany-list suggests fairly strongly that the deme belongs to the city-trittys,[99] and there is no evidence that Alcmeonid land came down to the coast, for the territory of Euonymon is in the way. No clear answer can be given. This is an area of old settlement,[100] with a very || primitive Demeter cult on Cape Kolias.[101] Perhaps here too there is a desire to detach a cult-site from its neighbourhood.

D Other religious organisations

The anomalies have indeed provided us with a clue, which suggests that it may be profitable to investigate the effect of the reforms on other religious organisations of a territorial character.

Let us begin with the Tetrakomoi. It is possible that they, like the Tetrapoleis, figured in Strabo's list of the twelve cities of Kekrops,[102] a list which certainly reflects some truth about early Attica. One name is missing, and the conjecture that their name has dropped out after that of the Tetrapoleis[103] is a plausible one. However this may be, the antiquity of their cult of Herakles is not in doubt. The four components are given by Pollux[104] as Piraeus, Phaleron, Xypete and Thymaitadai. The most likely interpretation of a fourth-century inscription shows Phaleron still possessing a κώμη-organisation,[105] and in 330/29 they still joined in a festival.[106] They are therefore a real unit with something in common. It is therefore interesting to find that Cleisthenes' reforms leave them in three different trittyes, Xypete going to Kekropis, Phaleron to Aiantis, Peiraeus and Thymaitadai to Hippothontis. It is reasonable to suppose that the splitting was intentional. We may go a stage further, and recall that Hippothontis has a trittys with an unrestored long name beginning with Τε or Ζε. It seems not unlikely that this should be Τε[τρακόμον], an earlier name for the Peiraeus trittys, which contains two of the old Tetrakomoi. The parallel with the Tetrapolis

[99] *IG* ii² 1742, as analysed by Loeper, *AM* 17 (1892), 388. [100] Mylonas, *Aghios Kosmas*.

[101] Plut. *Sol.* 8.4, Paus. 1.31.1; Nilsson, *Griechische Feste* 317. [102] IX.i.20, p. 397.

[103] See, e.g., Solders, *Die ausserstädtischen Kulte und die Einigung Attikas* 107–8, following Loeper.

[104] IV.105.

[105] *IG* ii² 1598. 9ff. See Roussel, *Revue Archéologique* 18 (1941), 226–31, though he thinks that the two κῶμαι which both have komarchs from Phaleron are sub-divisions of Phaleron, and I incline to think that one is Phaleron and one one of the other three components of the old Tetrakomon, which happens to be headed by a man or men on the Phaleron deme-register.

[106] *IG* ii² 3103.

would not be exact, since the Cleisthenic Tetrapolis really contained four demes and the Cleisthenic Tetrakomon should have at least five, Peiraeus, Thymaitadai, Koile, Keiriadai and Korydallos. Either the word Tetrakomoi would not be felt to have numerical significance, or perhaps we should regard Keiriadai or Korydallos as a splinter-deme of later origin.

Pallene is the other main candidate for the missing city of Kekrops,[107] but the League of Athena Pallenis presents greater problems since its composition cannot be precisely determined. The dedication quoted by Athenaeus[108] gives us three member-demes with relative certainty, Pallene itself, Gargettos and || Pithos, and the quotation from Themison which follows[109] assures us that there were a number of member-demes. Schlaifer[110] adds Acharnai to these from the quotation from the βασιλέως νόμοι which lies between these passages, but this seems an unnecessary addition, of no great topographical probability. It seems more likely that Athenaeus is simply quoting from Polemon a number of passages bearing on parasites.[111] Some of these certainly have nothing to do with Athena Pallenis, and I would add the passage on Acharnai to their number. Even so, we are left with a cult-league centred at Pallene, with the names of at least three demes which belonged to it. Under the Cleisthenic system there was a trittys named Pallene of elongated shape, from which Gargettos and Pithos were excluded. This seems to conform well to the pattern which we have established.

The Trikomoi,[112] on the other hand, present a different picture. Their three members, Eupyridai, Kropidai and Pelekes, are all in the same trittys. We know virtually nothing of the unit, but may surmise from its situation and the lack of references that it was of little importance. Nothing of importance emerges from an examination of the Mesogeioi[113] or the Paraloi,[114] attractive though their names are in this context.

[107] It will have dropped out at the end before πάλιν in the next sentence. See Solders, *Die ausserstädtischen Kulte* III.

[108] VI.234F. [109] VI.235A.

[110] *HSCP* 54 (1943), 35–67, a thorough discussion of the evidence for the League. (But see Additional Note.)

[111] See Jacoby's analysis of the passage, *FGrH* IIIb, i. 147–50.

[112] Steph. Byz. *s.v.* Εὐπυρίδαι; perhaps *IG* ii² 1213.

[113] Schlaifer, *CP* 29 (1934), 22–7, has all the evidence and establishes a *prima facie* case for considering them a *genos*. I am not very happy about this view. The likely demes represented are Bate, Diomeia, Kydathenaion, and Kerameis, all city demes, falling into three trittyes.

[114] A very puzzling organisation. *IG* ii² 1254 shows them with *epimeletai* sacrificing to Paralos in the Paralion, but it is tied to the state-trireme by the *tamias*.

Some minor points may be briefly noted. Kirsten[115] may be right to see significance in the isolation of the ancient city of Thorikos. There is some tendency to detach old cults of the tribal *eponymoi* from those tribes. The case of Erechtheus is clear. An ancient cult of Leos is attested at Hagnous,[116] which is nevertheless excluded from Leontis.[117] Similarly Erechtheis, Kekropis and Aiantis have to go outside their tribes to worship their tribal hero,[118] although Pandionis, centred on the Acropolis, and Hippothontis do not.[119]

E The purpose of the trittyes

Our entire argument so far has tended to show that demes were allotted to trittyes with some care, and that the trittyes were constructed in a deliberate attempt to create units which would be sufficiently distinct from existing local || units to compete with them and to destroy the influence which they gained from possessing a common cult in a common locality. Modern scholarship has tended to neglect the trittyes, seeing in them mere devices to ensure mixed composition of the tribes. This is an understandable tendency, since in the event the tribes won a success which can only be described as surprising, while the trittyes totally failed to compete with the older local organisations. There was a parallel failure in Cleisthenes' attempt to redefine the qualifications for citizenship. He put the deme beside the phratry, but could not persuade the Athenians that phratry-membership was of no importance. Pericles had to recognise this basic unwillingness to accept the substitute.[120] However, the fact that the trittyes failed to establish themselves as local units does not mean that they were not intended to have importance. The mass of fifth-century *horoi* from the Agora and from the Peiraeus indicates that they were intended to serve real purposes. The purposes are indeed mysterious. Raubitschek[121] has recently suggested uses for the Agora markers. The Peiraeus *horoi* presumably served as muster-stations on the lines that Demosthenes was trying to revive as late as 354.[122] The division of the *prytaneis* into trittyes survived into the time of the *Ath. Pol.*,[123]

[115] *Atti* see n. 52, 162. [116] ἀξόνες in Steph. Byz. *s.v.* Ἄγνους.
[117] Contrast the cult of Leos in the Cleisthenic deme of Skambonidai (*IG* i² 188 = i³ 244).
[118] *IG* ii² 1146, 1143; *Hesperia* 7 (1938), 18. [119] *IG* ii² 1138, 1149.
[120] This has been demonstrated by Andrewes, *JHS* 81 (1961), 13–14.
[121] *AJA* 60 (1956), 279–82. [122] XIV.22–3. [123] *Ath. Pol.* 44.1; cf. *IG* ii² 1748.

and the *epimeletai* of the tribes were selected by trittyes in the fourth century.[124] The trittyes retained property[125] and cults.[126]

Another substantial reason for modern depreciation of the trittyes has been their apparent random distribution on the map of Attica. Little attention has been paid to the enclaves, but a great deal of attention has been given to the contrary situation, where blocks appear to have been left untouched by the redistribution. The importance of these blocks can be overstressed. As Ehrenberg has said, Cleisthenes was interested in people, not territory.[127] But even the territorial arguments lose some of their force, when examined on a contoured map. Let us work through a recent list of these blocks.[128] It is not true that the inland and coast trittyes of Akamantis join. Thorikos is an enclave in Leontis, and the territory of Phrearrhioi lies between it and Kephale, the nearest point of the inland-trittys. In Pandionis, the coast and inland trittys touch at a point, not at a line, and the centre of population of the inland trittys is at Paiania, far to the west. In Aigeis, the trittyes are contiguous, but they are quite different in character. The coast-trittys is composed of coast-towns and plain; the inland-trittys is mostly hill, and has been deliberately || deformed. The case which is invariably quoted is that of Aiantis, where a flat map shows a solid north-eastern block. This of course was the case which made Beloch suspect Peisistratid influence, and a solid Peisistratid block in the north-east would contradict the general drift of our enquiry. But the case does not hold, either territorially or politically. The centre of population for the inland-trittys is at Aphidna, well up in the hills, a long and tedious climb from Marathon, while the evidence for interference with the composition of the coast-trittys is clear. Politically, it may indeed be true that Marathon remains a centre of Peisistratid influence. Can this really be the case with Aphidna, where we know of two leading families, the Gephyraioi, the family of Harmodios,[129] and Kallimachos the polemarch who led the Athenians at Marathon?[130] If Aiantis was a Peisistratid-packed tribe, they had forgotten it by the time they led the right wing in 490.[131]

[124] So in *IG* ii² 1151, 1152, 2818. I retain a suspicion, however, that [ἐπιμελητ]αί is the correct restoration in *IG* ii² 2824, where all three come from the same deme. No-one has ever produced an alternative which is plausible.

[125] *IG* ii² 2490. [126] *IG* i² 190 = i³ 255, ii² 1172. [127] *Neugründer des Staates* 90.

[128] Hignett, *History of the Athenian Constitution* 134. [129] Plut. *Quaest. Conv.* 1.628D.

[130] Her. VI.109. [131] Plut. *Quaest. Conv.* 1.628E.

There is indeed a certain contiguity of trittyes, but this is to be expected if the trittyes were, as we are told,[132] assigned to the tribes by lot. Provided that the trittyes were properly constructed in themselves, any random allocation would do. The only necessary qualification was that each tribe should have, a city, a coast and an inland trittys, ὅπως ἑκάστη μετέχῃ πάντων τῶν τόπων. The trittyes cut across the most obvious local loyalties. It could be left to Apollo to create the tribes, to produce the wider loyalties to be implanted when men served together on the *boule* and, more importantly, in the army. In the event, the men of Phaleron fought side by side with those of Marathon and Aphidna, and Leontis united Sounion, Skambonidai, and eastern Parnes. Eleusis, Kerameis, and Aphidna might have been better still, but the lot would produce adequate results, and in fact it taught the right lessons.

III CLEISTHENES

We have left some parts of the reform in the dark for want of evidence. This lack comes partly from the paradox I began with. We know about local cults, because they survived, because Cleisthenes failed with them. Other institutions did not retain their hold, and we know so little of the old tribes, the old trittyes and the old naucraries that our picture may have been substantially distorted. The naucraries in particular may well have had territorial implications[133] and Cleisthenes does not seem to have abolished them.[134] But there are other patches of darkness. Some have recently seen in Cleisthenes' moves a desire to end the old threefold distinction of caste.[135] Many have found || the evidence that Cleisthenes desired to bring in new citizens unconvincing.[136] Perhaps most important of all, we have next to no evidence on the confiscation and redistribution of land in sixth-century Athens.[137] The two

[132] *Ath. Pol.* 21.4.

[133] The one name we possess, Kolias, suggests as much (Bekker, *Anecd.* i. 275).

[134] Kleidemos, 323F8.

[135] Wüst, *Historia* 3 (1954–5), 137–9, 6 (1957), 176–91, 8 (1959), 1–11, accepted by Oliver, *Historia* 9 (1960), 503.

[136] Wade-Gery, *Essays in Greek History* 148–9, but more firmly and less convincingly Oliver, *Historia* 9 (1960), 503, who, in order to maintain not only that there was no admission of new citizens, but that no-one ever said that there was, has to offer an impossible meaning of ἐφυλέτευσε and an extremely unlikely one of νεοπολῖται.

[137] The only pieces of evidence worth considering are these: (1) redistribution is said to have been in the air at the time of Solon's reforms (Plut. *Sol.* 16.1); (2) Peisistratus' enemies confiscated his property and auctioned it while he was in exile (Her. VI.121.2);

things which emerge most clearly are an attack on organisations which held a locality by religious ties, some of them in areas attached to political opponents of Cleisthenes, and an attempt to unify Attica by making men from different areas work and fight together.

How far was Cleisthenes a politician, how far a statesman? The Alcmeonids in the sixth century had been caught between the fires of the autochthonous landed population of the plain, secure in the possession of the most important state cults, and their fellow-Pylian Peisistratus, who in the east had built himself a remarkable coalition of varied interests and had secured himself in the tyranny by promoting economic prosperity and by giving Athena wider appeal without the aid of her traditional guardians. The Alcmeonids possessed no important local cult of their own that we know of. Their power rested solely on land and wealth, which they buttressed by looking outside Athens to Delphi, Sicyon and Lydia.[138] In the struggle for power before Peisistratus, they had been squeezed out and forced to rely on their position outside Athens. Cleisthenes had watched this process under his father Megakles and Megakles' successor, his uncle Alkmeonides.[139] The veering of tyrannical favour which brought him home and to the archonship in 525 made no essential difference to the lesson. He had learnt the power of local cults and the value of a wider Athens.

When he came home finally in 510, the position had been transformed by Peisistratus and his sons. New classes had been given economic hope, and men had learnt to look to Athens as the undisputed centre. There were features || here which he may well have disliked and which were certainly disliked by other men of power. A διαψηφισμός cut into the tyrants' enfranchisements.[140] Other moves may have cut into their land-distributions.

(3) Peisistratus made money-grants to farmers (*Ath. Pol.* 16.2); (4) when he recalled Kimon Koalemos from exile, Kimon κατῆλθε ἐπὶ τὰ ἑωυτοῦ (Her. VI.103.3); (5) Peisistratus' friend Lygdamis of Naxos did indulge in confiscation (Arist. *Oec.* II.1346B7). It is clear that Peisistratus had the power to confiscate and it would have been surprising if he had not used it, but the positive evidence for saying so does not exist.

[138] I do not wish to be understood to say that they had mercantile interests. I know of no evidence for that proposition, widely held though it is.

[139] I see no good reason to disturb the manuscript reading at Her. 1.64.3. For Alkmeonides, see Raubitschek, *Dedications from the Athenian Akropolis* 338–40, where the evidence is collected. I cannot follow him in supposing that there is any strong case for supposing Anaxileos a member of the family.

[140] *Ath. Pol.* 13.5.

Cleisthenes' main opponent came from Pentelikon, deriving strength from the close-knit religious unions of that area, perhaps from the prestige of the new dramatic festivals of Ikaria.[141] Cleisthenes had Delphi behind him, but so far Delphi had only been of use as a lever to bring in Sparta, and Cleomenes of Sparta backed Isagoras. Isagoras, with Spartan backing and using the methods of the 560s, won the upper hand.

If there was to be a place for the Alcmeonids in Athens, new methods had to be used. One lesson could be learnt from the tyrants, and Cleisthenes turned to the *demos* which the tyrants had created and favoured, which he and other dynasts had previously rejected. It is hard to see how the complexities of the reforms could have been used in themselves as a bargaining-counter to win the *demos*, even at the lowest level, the prospect of local self-government in the deme. Something more concrete will have been in the air – citizenship, probably also land. Land-hunger remains strong in Athens for the next fifteen years or so. Salamis,[142] Chalkis[143] and Lemnos,[144] all are attempts to meet a need which Cleisthenes could only partly satisfy in Attica, since land to satisfy it could only come from his opponents, not from his supporters.

At this point constitutional problems have been multiplied. How could the reforms have been put through while Isagoras was archon, able, as presiding officer, to block a vote? Was Cleisthenes an archon? Did he hold a special commission? How could he have obtained a majority, if his supporters had been disfranchised? These problems seem irrelevant to a revolutionary situation. Whether Isagoras was in the chair or not, whether the *neopolitai* were legally entitled to vote or not, there was nothing to prevent them carrying clubs and standing round or even on the Pnyx to shout 'All power to the ten tribes.'

Cleisthenes needed a new system. The διαψηφισμός had shown the difficulty of providing a legal basis in the phratry-system for doubtful citizenship, and to intervene directly in the phratries might alienate as many voices as it won. The creation of the deme met this difficulty, but there still remained the problem of local vested interests. This could be met by establishing the trittyes, which might curtail the powers of local organisations. The real problem came in deciding how to crown the pyramid, how to ensure

[141] Pickard-Cambridge, *Dithyramb, Tragedy, and Comedy* 97–104 (2nd edn, 69–76).
[142] Tod II. [143] Her. V.77.2. [144] Her VI.140.2; *BCH* 36 (1912), 329–38.

that the new local units served interests wider than local ones and looked to Athens in the way Peisistratus had shown. The four Ionian tribes seem now to have represented very little.[145] Changing tribal organisation was not unparalleled in the sixth century. || Cleisthenes' own grandfather, Cleisthenes of Sicyon, had changed tribe-names, and may have created a new tribe from the under-privileged.[146] At any rate, he had a tribe for the previously under-privileged, and asserted the principle that it was better than the others. At Cyrene, Demonax of Mantinea had acted rather differently.[147] Finding the original settlers organised in the three Dorian tribes and the newcomers not organised at all, he created a new three-tribe system on ethnic lines. These systems had given new citizens or settlers rights, but retained the seeds of difference and dissension. Cleisthenes realised that there was no possibility of peace and concord without mixing his disparate elements. The novelty of his solution lies in its determination to make a fresh start. All citizens, old and new, would start equal in his new demes and new tribes.

This is his most important service to Athens. Traditionally, and perhaps temperamentally, averse to tyranny, he found a way to create a pyramid of power which would have at its head, at least in theory, the people and not one person, and which would maintain the unifying force generated by the tyranny without a tyranny's disadvantages. The way he chose had distinct advantages for himself and his family. The details of his settlement could be and were manipulated to the disadvantage of traditional opponents, but the nature of the Alcmeonid position was such that it could not be affected by this settlement. The trittys-lines were drawn to leave Alcmeonids in at least three different tribes, a result which might well prove fatal to a family which depended for influence on the control of a local cult, but which could be positively welcomed by a family of land and wealth which acquired the opportunity to have a hand in the affairs of three of the new tribes. But it cannot be denied that, politician though he was, Cleisthenes was capable of seeing the

[145] I deduce this only from their lack of success in surviving Cleisthenes. One sacrifice (*Hesperia* 4 (1935), 5ff.) and the *phylobasileis* (*Ath. Pol.* 57.4) exhaust the evidence.

[146] Her. v.68. That the Ἀρχέλαοι were a new tribe seems to follow from the fact that a new name has to be found for them when the other tribes reverted to their Dorian names. Αἰγιαλεῖς is explicitly pro-Adrastus and therefore anti-Cleisthenes.

[147] Her. IV.161.3; cf. Chamoux, *Cyrène sous la monarchie des Battiades*, 138–42, Jeffery, *Historia* 10 (1961), 142–4. Miss Jeffery's preferred explanation, that Demonax, like Cleisthenes, included a cross-section *inside* each of his three tribes, does not seem to me a possible interpretation of the text.

advantages of Attic unity. ἀνάμιξις would have been the key word in his thought, as it was in the language of his most sympathetic commentator.[148] ἔτι δὲ καὶ τὰ τοιαῦτα || κατασκευάσματα χρήσιμα πρὸς τὴν δημοκρατίαν τὴν τοιαύτην, οἷς Κλεισθένης τε Ἀθήνησιν ἐχρήσατο βουλόμενος αὐξῆσαι τὴν δημοκρατίαν, καὶ περὶ Κυρήνην οἱ τὸν δῆμον καθιστάντες. φυλαί τε γὰρ ἕτεραι ποιητέαι πλείους καὶ φρατρίαι, καὶ τὰ τῶν ἰδίων ἱερῶν συνακτέον εἰς ὀλίγα καὶ κοινά, καὶ πάντα σοφιστέον ὅπως ἂν ὅτι μάλιστα ἀναμειχθῶσι πάντες ἀλλήλοις, αἱ δὲ συνήθειαι διαζευχθῶσιν αἱ πρότερον. Every increase in our knowledge shows that Cleisthenes worked towards this aim.

Additional note. In discussing the League of Athena Pallenis, I forgot the fourth-century inscription *AM* 67 (1942), no. 26, pp. 24–9. This complicates the matter, since it shows that *parasitoi* then came from a very wide area and makes the *parasitoi* previously known unsafe guides to the composition of the League, which may or may not have been narrower originally. The four *archontes* of the inscription *may* be a safer guide; they come from Gargettos, Acharnai, Pallene, and Paiania, all four from different trittyes and tribes.

[148] Twice in *Ath. Pol.* 21, apart from the passage which follows, which is *Politics*, VI.1319B19.

II

Review of *The Political Organization of Attica*

A Study of the Demes, Trittyes, and Phylai, and their Representation in the Athenian Council

by John S. Traill

This volume grew out of the work now published as *Athenian Agora* xv, *The Athenian Councillors*, which contains its primary epigraphic evidence. It effectively replaces a great deal of earlier work, and will be needed wherever Athenian institutions are studied.

Traill establishes that the number of constitutional demes in Cleisthenes' system was 139. One new deme was created on or soon after the foundations of each of the tribes Ptolemais, Attalis and Hadrianis. Other demes attested in ancient evidence or hypothesised by scholars are discussed and rejected, except for some used quasi-officially in lists of the Roman period.

The evidence here collected suffices to establish the representation of these demes in the Council until about 200 BC. The evidence is still unsatisfactory for the tribe Kekropis, and figures for Hippothontis before 307 BC have to be partly inferred from later evidence. There was no redistribution of representation until the foundation of the Macedonian tribes in 307 BC. After 200 BC fixed quotas of representation were abolished.

Traill makes a substantial attempt to determine the relation of representation to actual population; a similar, more speculative, treatment, not used by him, has been made by P. J. Bicknell, *Studies in Athenian Politics and Genealogy* (*Historia* Einzelschrift 19, 1972). Apart from some rather doubtful evidence from ephebe-lists, the comparison still has to be with tables based on Kirchner's *Prosopographia Attica* of 1898–1901. Traill points out some

* Published in *AJA* 80 (1976), 311–12.

ways in which these tables could be misleading, and more could be added; I do not think the comparison serves more than very general purposes.

Traill offers a drastic and convincing simplification of the methods followed to construct the five post-Cleisthenic tribes, which completely supersedes all previous treatments. Antigonis and Demetrias were created with the primary intention, which could not be fully carried through, of taking three demes from each pre-existing tribe. Ptolemais, Attalis and Hadrianis were all formed by taking one deme from each pre-existing tribe and adding a new deme. Not much was done to redistribute quotas at the foundation of the Macedonian tribes or that of Ptolemais; the most substantial exception is the doubling of the quota of Lower Paiania from 11 to 22, apparently in 307 BC. I do not think that Traill fully takes the point that, since the size of the Council rose from 500 to 600 in that year, a deme of which the quota remained the same had its proportionate representation reduced. There had to be a whole number of councillors for a deme; a quota of 1 could not be increased to 1.2, hence further anomalies.

A full bibliography and excellent maps (which can be obtained separately) provides the best guide to deme-locations currently available. Perhaps add my remarks, *Historia* 12 (1963), 31–2 = this volume, pp. 88–9, to the discussion of Hekale; in the separate discussion (p. 29 n. 12) of the location of Berenikidai, I judge it more likely that *IG* ii^2 1221 belongs to the military context of *IG* ii^2 1304 than that it is a deme-decree. On the problems of the sub-units of Aphidna which acquire a semi-independent existence (pp. 87ff.). I have written in M. I. Finley (ed.), *Problèmes de la terre en Grèce ancienne* 192–93, 205 = this volume, pp. 269, 283–4; they had a real existence under komarchs even in the fourth century.

The assignment of demes to trittyes is more difficult, as Traill readily admits. Pp. 44–46 n. 18 belatedly transfers two Potamos demes to the city-trittys of Leontis, and the Tables have to be corrected accordingly. Krioa may well have to move to the city-trittys of Antiochis, and several small demes are unplaceable. However, the overall pattern is clear. City-trittyes tend to be small and provide only just over a quarter of the members of the Council. This is an enlightening fact for the settlement-pattern of Attica in the late sixth century. It might be thought surprising that there was no visible pressure for later redistribution, particularly if it were really true that the Council was a 'representative government,' a phrase incautiously borrowed by Traill from Larsen, who himself only thought it applicable to a few years

after Cleisthenes. However, the hereditary nature of deme-membership will have meant that the under-representation of city-residents was more apparent than real.

The only fundamental criticism which can be made of this scrupulously careful work is implicit in its title. No word in it suggests that Cleisthenes' reorganisation was also a military reorganisation and that || the Athenians habitually fought by tribes and were occasionally mustered by trittyes. There are problems implicit in the debate over the population of Acharnai (Dow, *TAPA* 92 (1961), 66–80; Thompson, *Historia* 13 (1964), 400–13) which have not been properly faced.

12

Review of *Die Trittyen Attikas und die Heeresreform des Kleisthenes* by Peter Siewert

'Es ist alles eitel Gold' said Wilamowitz of Aristotle's treatment of Kleisthenes' reforms in the *Athenaion Politeia*, complaining only that there was not more of it. For Siewert the proposition that Kleisthenes divided the country into thirty parts, ten round the city, ten in the coast, ten in the inland, called them trittyes and assigned them to the tribes by lot is unequivocally false, a very casual fourth-century deduction from the names of the trittyes without inspection of their actual nature. As for the statements or implications that Kleisthenes wished to mix up the population or introduce new citizens, they do not appear to be worthy even of discussion. Herodotus' view of the nature of the reforms gets even rougher handling; the whole of v.69.2 has vanished without trace. Instead the reforms have become a largely military reorganisation, designed to improve the state's war-machine, probably in the interests of the upper classes.

That military motives were primary in the reforms has been suggested from time to time (see S. p. 9 n. 44). That they were not considered at all in Traill's fundamental *Political Organization of Attica* (*Hesperia* Supp. 14 (1973), henceforth *POA*) was a legitimate complaint (cf. *AJA* 80 (1976), 311–12 = this volume, 101). S. argues that the primacy of military motives can be deduced by a detailed study of the trittyes, in relation to the road system and in the detectable effort to ensure their numerical equality. As a consequence of the last, further study will be necessary to establish that it was equality of hoplite numbers, not of citizen numbers, which Kleisthenes

* Published in *Gnomon* 55 (1983), 431–6.

aimed at, a possibility which has occurred to others, at least at the level of the tribes (P. J. Rhodes, *A Commentary on the Aristotelian Athenaion Politeia* 253).

The trittyes had little attention before the discovery of the *Athenaion Politeia*. After the discovery the task seemed to be to define their geographical limits. This was true even of Eliot, *Coastal Demes of Attika*, but he was the first who could use improved information about the representation of demes on the boule, and therefore raised problems about the varying populations of the trittyes, denying that they could have been assigned to the tribes by lot. More or less at the same time I (*Historia* 12 (1963), 22–40 = this volume, pp. 77–98) doubted the uniformity of the geographical criterion in their makeup. A new phase opened when W. E. Thompson (*Historia* 15 (1966), 1–10) called attention to the fact that some, by no means all, lists of || prytaneis and bouleutai seemed to be organised, not by geographical trittyes, but by more even divisions which he identified with the τριττύες τῶν πρυτάνεων referred to in *Ath. Pol.* 44.1. That case was developed by himself and others, and finally Traill, who had paid little attention to it in *POA*, not only accepted it, but began to raise the possibility that these trittyes were not fourth-century creations, but the original trittyes of Kleisthenes himself (*Hesperia* 47 (1978), 100–9). This is the position which S. endorses and develops.

Traill has now developed the position on his own account in *Hesperia* Supp. 19 (1982), 162–71. Significant patterns, i.e. recurrent groupings which transcend geographical limits, are now attested for tribes II, III and IV. We should note that what Traill and S. are saying is that it is possible to hypothesise similar divisions of other tribes, except for VI, into groups contributing 17 + 17 + 16 bouleutai, though such divisions may only appear once, if at all, in surviving lists. It is alarming to find that, for *The Athenian Agora*, XV 42, the text which set Thompson going, S. holds in one case and Traill now in two that these modified trittyes have had their order upset by grouping all demes of a similar name (Potamioi, Lamptreis) together. It may well be thought a long step to deduce from phenomena originally thought to show fairly regular groupings designed to produce equal groups of prytaneis in three tribes that Kleisthenes himself designed all tribes (except Oineis) to be divided into groups of 17, 17, 16. The test must be in the ability of the deduction to solve other problems.

S.'s introductory chapter (1–35) sketches all this and much more. There is a particularly useful section on the inscribed *horoi* which mention trittyes,

but the unfamiliar element is the emphasis placed on what is known of the road-system of Attica.

It is the roads which play the principal part in Teil 1 (38–86). The first section takes its start from an observation, first made by Sauppe in 1846, that the city-trittyes, or part of them, seem to be arranged, anticlockwise, in the official order of the tribes. Siewert argues that they should be thought of as sectors, essentially radial slices of the clockface, and that the demes in them are connected by roads. He sees a close similarity between this and the state of Plato's *Laws* where the whole country seems to be divided into twelve radial sectors (745B–C, 873D). He manages to find such sectors for five tribes (VIII, VII, VI, V, I) and runs into totally intractable problems on two (II, IV); three city-trittyes (III, IX, X) only have one deme in any case. A discussion of whether the Agora or the Akropolis (so in Plato) is the centre of this system ends in favour of the Agora.

I confess to not seeing much in any of this. There may be some circularity in the deme identifications, a point to which I return. The point about roads is almost a truism; given two places, there must be a means of getting between them. Two of his sectors (VI, I) could equally well be explained as geographically bounded. He gets into so much trouble with Leontis (IV) that he comes to consider its arrangement an afterthought, a sign of an unsatisfactory hypothesis. As for Sauppe's original idea, it is a cause for disquiet that the sequence begins with Erechtheis, a tribe with no inner-city deme, and we have no evidence that the official order of the tribes goes back to 508.

The second section is much more seductive. That some inland and coast trittyes touch has long been a puzzle, leading Beloch, for example, to the view that the reform was Peisistratid, not Kleisthenic. Eliot had suggested that it was deliberate, || without explaining its purpose. S. knows its purpose. In the relevant tribes, forces being mobilised would come in along the same roads, gathering strength as they moved to Athens. The relevant trittyes of Aiantis (IX) would meet somewhere west of Pentelikon, at least if those starting from the Marathon area took the more difficult inland route rather than the easier road south of Pentelikon, not a totally reasonable preference (cf. e.g. Burn in *Greece and the Eastern Mediterranean . . . Schachermeyr*, 90–1). Most of the relevant demes of Aigeis (II) can be arranged along two roads; Milchhöfer had observed this in 1887, but dropped it after the publication of *Ath. Pol.* All relevant demes of Pandionis (III) (except Probalinthos, a problem for everyone) fall neatly along a road attested for

the sixth century by Plato, *Hipparchus*, 229A, and most of those of Akamantis are along a clear route. A further attempt to link these cases up with the relevant city-trittyes really gets nowhere; it can hardly be relevant whether a tribe is united for its last mile, last hundred yards or not at all. Some other trittyes show a tendency to be aligned on a central road; one would think this is hardly a surprising consequence of contiguity.

This is by no means unimpressive. The really satisfactory point is that it provides an explanation of a recurrent grouping of demes in Aigeis (11) which had puzzled Thompson (*SO* 46 (1971), 78–9) and, still more, Traill. I think it unlikely that even the most convinced Aristotelian will continue to insist that Philaidai, wherever precisely it was, has to be grouped as a coast-deme with Halai Araphenides rather than an inland deme connected by road with Erchia and Gargettos, with which it appears on the lists.

It will also be hard to insist that the placing of these four tribes was achieved by lot. Rough calculation, not attempted by S., suggests that the chances of this pattern arising accidentally are 1 in 5,040. There may be more cause for disquiet about the thoroughness of S.'s work on the roads. Despite the prominence of Zentralwege in the book, there is no reference to the three instances of ὁδοὶ ἀστικαί in epigraphic texts. Two will cause him no difficulty (*Hesperia* 6 (1937), 455 no. 5, line 12; *IG* ii^2 1582.116, on which see Young, *Antiquity* 30 (1956), 97 with pl. Va; Eliot, *Coastal Demes* 100). The third is *Hesperia* 19 (1950), 210 no. 4, line 24, a broken reference to ἡ ὁδὸς ἡ] ἀστικὴ Βήσαζε [φέρουσα. S.'s Map IV has no road conceivably to be so described, and the inevitable route for it would take the Besaieis north to Amphitrope and Kephale rather than south to their fellow-tribesmen. Perhaps it was built later, for the mines, and not available in 508.

Teil II (87–138) begins by re-examining the 'Prytanen-Trittyen' along the lines of Traill's 1978 paper. He starts with Aigeis (11). After the re-examination of Teil I, it now shows a well-attested 'Prytanen-Trittys' arranged along a road, providing 17 bouleutai. The coastal 'Prytanen-Trittys' is also now better understood geographically, but it regularly shows the city-deme of Diomeia, which brings it up to 16 bouleutai; Diomeia is to be classified as a 'quasi-enclave', that is, a deme in geographical contact with other demes of its tribe but assigned to another trittys. The city-trittys, thus deprived of a one-bouleutes deme, would only have 11 bouleutai, had not the demes of Ikarion (5 bouleutai) and Plotheia (1 bouleutes), isolated north of Pentelikon, been added to it, as they appear on two lists. Aigeis is thus

found to break all traditional rules. Its inland-trittys contains a coast-deme, if Philaidai is to be accounted such; its coast-trittys has a city-deme; the city trittys has two inland demes. S. plots hypothetically the various motives of sector-order, road-connection and trittys-equality, which could have led to such a result, without being able to explain everything. Similar operations are then applied to other tribes. The enclave Probalinthos becomes part of the city-trittys of Pandionis. || Rhamnous joins Phaleron in the city-trittys of Aiantis. Phyle goes to the city-trittys of Oineis, and so on. Multiple transfers, as in Aigeis, are found in Antiochis and Erechtheis.

I do not attempt criticism in detail, nor do I see the need here to distinguish between S. and Traill. Traill's 1982 paper now gets a different solution for Antiochis by reversing the positions of Aigilia and Phrearrhioi, and it has a much improved solution for Erechtheis, resting on an interpretation of abbreviated *horoi*; the same interpretation has been found by Lauter, *AA* (1982), 299–315, but his conclusions are different. On the general nature of the operation, I repeat that the 'Prytanen-Trittyen' are only attested in three tribes so far, and reserve a major qualification about deme-locations.

Up to this point, although S. has sometimes fallen into language suggesting that he is dealing with the origins of the trittys-system, nothing has yet been formally said to suggest that we need be dealing with anything but the fourth-century τριττύες τῶν πρυτάνεων.

The transition comes with the study of the enclaves, claimed by me in 1963 as likely to offer a key to Kleisthenes' purposes. I then suggested that their purpose was to break up old cult-organisations, a view that S. argues is unfounded, with some very double-edged arguments. (On p. 119 he argues that the Marathonian Tetrapolis remains compact, without saying how he would deal with someone who might argue from his facts that Kleisthenes was suspicious of its loyalty and trying to encircle it.) He takes up a suggestion, made by Eliot in the case of Probalinthos, that the purpose was to equalise trittyes. Operating with a vastly increased list of enclaves, either physically separated from their tribes or regarded as movable, he proceeds to argue that their origin must be due to this motive and to no other. They reveal a passion for equality of trittyes, most clearly seen in the quasi-enclaves, a compromise between the desire for equal trittyes and the preference for contiguity of geography and communications.

I find it very difficult here to disentangle anything like a summarisable argument. S. has simply been granted an insight into Kleisthenes' mind and

takes it from there. Since by now he is working with the city Sektorenordnung, road-communications, ordinary geography, uncertain deme-locations and uncertain bouleutic quotas, and Kleisthenes must be allowed to have balanced one consideration against the other, it is not surprising that he finds it possible to account for the phenomena. He raises the possibility of a computer program which might sift the possibilities. That is perhaps hardly necessary. Professor G. R. Stanton has, for example, shown me a scheme for south-east Attica which fulfils S.'s purposes of equal units moving to Athens along fixed routes a great deal more economically and efficiently than that which S. attributes to Kleisthenes.[1] It might however be thought to have one major disadvantage for Kleisthenes. It contains nothing like the large coastal trittys of Antiochis for which Alcmeonid influence can be plausibly argued, which we now have. But S. has ruled out the possibility of making any inference from such a phenomenon.

One point should be noted which comes close to a formal refutation. S. has transferred Plotheia to the city-trittys of Aigeis, for Kleisthenes as well as for the Prytanen-Trittyen. In *IG* i³258.30–1 Plotheia acknowledges that it may have cult-responsibilities ἢ ἐς Πλωθέας ἢ ἐς Ἐπακρέα[ς ἢ ἐς Ἀ]θηναίος. Since *IG* ii² 2490.8 names an Ἐπακρέων τριττύς and the name has always been thought to point to the Pentelikon region, the standard, indeed the almost inevitable, view has been that this was the inland-trittys of Aigeis and that Plotheia belongs to it. S. brushes it inconspicuously aside on p. 15 n. 67, with a reference to a note by Thompson, who used a parallel which was not a parallel. ||

Teil III (138–153) pulls the threads together. The two principles which prove the military origins of the reforms are the relation to the road-system and the striving for equal units. Of roads I have already said enough; there is something here, but many countercases. For S., following others, notably Bicknell, the Kleisthenic army was 9,000 strong, composed of ten tribes of 900, each composed of lochoi of 300; every trittys provides such a lochos. Military use of trittyes is attested, at least for the fleet, by fifth-century *horoi* from the Piraeus and Dem. XIV.22–3.

The closest parallel to Athens is the Boeotian League, with its eleven μέρη, each of which provided, not only 60 bouleutai, but 1,000 hoplites, each such group perhaps divided into three (the evidence is totally unsatisfactory).

[1] [Subsequently published in *Chiron* 14 (1984), 1–41 at 5–6.]

The Kleisthenic system provided thirty lochoi, moving to the Agora along the road-system, gathering strength as they went; that, for example, the Rhamnusians went all the way to Athens before they met the rest of their lochos from Phaleron is apparently not important.

There is in fact no evidence that the trittys was ever a constituent of the Athenian army, and search in areas not explored by S. gives him no comfort. Neither the marines nor the citizen sailors of *IG* ii² 1951 show any sign of organisation by any sort of trittys. What of the fourth-century ephebic lists, tribally organised? None of those currently recognised shows any such pattern, though *IG* ii² 2401, which I shall shortly republish as a fragment of one, has three demes which correspond to Traill's new arrangement of Erechtheis. More strikingly, the fourth-century ephebes are commanded by a cadet taxiarch and cadet lochagoi. No tribal list has as few as three lochagoi; no complete list of lochagoi is a multiple of three.

Teil IV (154–167), on the historical background to the reforms, is a very unsophisticated piece of work. Given S.'s views on the *Athenaion Politeia*, his belief that the tyranny disarmed the Athenian citizen population for good required more argument. As the omissions noted in my first paragraph indicate, he has followed those modern scholars who have thought that any suggestion that Kleisthenes lacked the support of the nobility is later propaganda. That makes it possible for him to hold that Kleisthenes' planning of the reforms, now more complicated than ever, perhaps began as early as 510. This, of course, to get the new army into shape for its successes of 506.

The first published response to S. (Lauter, *AA* (1982), 314–15) makes the fundamental point that Traill and S. have, without fully realising it, removed one of the major tools with which the Demenforschung on which they rely has hitherto operated. The bouleutic lists can no longer be relied on as indices of geographical location. All the work on deme-locations summarised by Traill in *POA* now needs scrutiny to see how far it was dependent on these lists.

To take Leontis, where Traill and S. agree. The Prytanen-Trittyen are now clear enough, but there have always been too many demes unplaced geographically, and neither of them really notices how much the locations have depended on the lists, particularly in the cases of Upper and Lower Potamos, Cholleidai and Oion.

I add another case. The identification Leukonoion = Peristeri is not overwhelming (Traill, *POA* 44, 134), though S. accepts it without question.

What, however, of *IG* ii² 1582.34, a property at Anaphlystos bounded by a road to Λευκο⟨ν⟩οῖον (ΛΕΥΚΟΟΙΟΝ ect. Kirchner; my squeeze confirms the length of the name, but leaves me in doubt about the second omikron)?

Would not this have been accepted as evidence for the location if the prytany-lists had not seemed to dictate that it was a city-deme? May not the relation between the coastal trittyes of Leontis and Antiochis be even more complex than it seems at the moment?

In this area Lauter has shown that even as careful an operation as Eliot's is vulnerable to even more thorough examination of sites and to scepticism about || Strabo's method of work. Deme-locations are crucial, and S. and Traill have reduced, not enhanced, our confidence about them.

Though flawed in this way and by over-confidence, S.'s book is a substantial contribution to Kleisthenic studies which will give the reader much to think about and which stresses points too often overlooked. I remain of the opinion that, if we look at Kleisthenes in the context of other Greek tribal reforms, we should expect that the composition of the citizen body was in some sense at issue. I do not think that political considerations can be put out of our minds quite as easily as Siewert thinks. I am becoming deeply sceptical about the mathematics of bouleutic quotas, and doubt whether the alternative of basing them on hoplite numbers rather than citizen population will solve all the problems. Problems about the reforms are likely to remain with us, and there is so much scope for ingenuity over detail that fresh claims to have found the key can be confidently awaited. We shall be lucky if they are all as interesting and fresh as this one.

13

The Kerameikos ostraka

This is a strictly provisional note, to voice a doubt about some of the conclusions which are already being drawn from the major find of ostraka in the Kerameikos.[1] There is no prospect of any fully rational discussion until the final publication, but I think it worth pointing out that, at any rate on the published evidence, there is a possible alternative explanation of the find. This explanation is not without difficulties of its own, but I hope to show that the current orthodoxy is not free from difficulty either.

I leave out of account the upper layer, containing the ostraka of Hagnon, Thucydides, Kleippides and, presumably, Perikles. There is no doubt that these ostraka belong to the 440s and only serve as a *terminus ante quem* for the sealing of the lower layer. Willemsen's view of the lower layer is that it contains ostraka from before and after 480 so inextricably mixed, with later ostraka found below earlier ostraka, that its internal stratification is valueless.

It is relatively clear what has led Willemsen to this view. He has assumed that all Megakles ostraka belong to his known ostracism in 486 and also that the ostraka of at least Dieitrephes and the elder Alcibiades belong well after 480, since Vanderpool has demonstrated this for their Agora ostraka (*Hesperia* 21 (1952), 1–8; 37 (1968), 118–19). He originally (*AM* 80 (1965), 102ff.) also accepted Raubitschek's date of 458 for the ostracism of Menon (*Hesperia* 24 (1995), 286–9), but I gather that he now prefers a slightly earlier date. Acceptance of these postulates inevitably implies that the deposit is mixed.

* Published in *ZPE* 14 (1974), 1–4. The suggestion made here was made independently by P. J. Bicknell, *AC* 44 (1975), 172–5; rejected by G. M. E. Williams, *ZPE* 31 (1978), 103–13; accepted by Willemsen (cf. 'Megakles and Eretria', pp. 114–15, below).

[1] Bibliography: F. Willemsen, Ἀρχ. Δελτ. 23 (1968), β. 1, 24–9; H. B. Mattingly, *University of Leeds Review* 14 (1971), 280–7; Thomsen, *The Origin of Ostracism*, 69–80, 92–108. I am also grateful to H. B. Mattingly and E. Vanderpool for advice and scepticism, and to Vanderpool for notes of a lecture given by Willemsen in Athens on 13 December 1973.

I make one quite general point about the postulates. In the study of ostracism before the discovery of ostraka, it was freely assumed that only two or three people were involved in any one ostracism. Since one of the main contributions of actual ostraka || has been to show that a very large number of people might have votes cast against them in any one year, it is surprising still to find a tendency to assume that ostraka belong to a year when the person was ostracised (an extreme example in Thomsen, 98). In fact, we cannot properly exclude any year in which the person may have been in Athens, and there is nothing improbable in supposing that some people may have been 'candidates' over a period of twenty to thirty years.

Specifically, acceptance of the postulates means that we have to believe that there was a dump of ostraka from the ostracism of 486 (possibly also that of 485) which remained unsealed by Persian destruction and post-Persian tidying up. At some stage, this dump became confused with ostraka from a vote or votes from the late 470s and 460s and confused to such an extent that the combined collection was dumped quite indiscriminately and buried before the upper layer went on in the 440s. I would not describe this as an impossible process, but it is certainly a complicated one. Archaeologically, it would seem best to see whether the stratification cannot in fact be trusted. Is it impossible to believe that the deposit is a unified one?

The cardinal reason for distrusting the stratification lies in the belief that the Megakles ostraka have to belong to 486. I submit that this is an unnecessary belief. There is after all one precise statement (Lysias XIV.39) that Megakles was ostracised twice. Carcopino, *L'Ostracisme athénien*, 112–13 and Raubitschek, *TAPA* 79 (1948), 204 have tried to explain it away, but the proposition is at any rate not chronologically impossible. Megakles was still a fairly young man in 486, and his son's known activity falls in the 430s and 420s (Davies, *Athenian Propertied Families* 379, 381).

The Kerameikos ostraka themselves argue against 486 for the date of the Megakles ostraka contained in them. It is surely a reasonable working hypothesis to suppose that ostraka which came from the same pot were used on the same occasion. As far as I know at the moment, Megakles is by this argument linked to Themistokles, Aristides, Kimon, Mnesiphilos Phrearrhios, Hippokrates Anaxileo, Leagros and Kallias Hipponikou. Themistokles, Aristides and Hippokrates Anaxileo are known from Agora closed deposits to have been candidates in the 480s and no one, except those who believe Themistokles was a new politician in 482, would be surprised to find them

'candidates' in 486. || But Kimon, Mnesiphilos, Leagros and Kallias Hipponikou are not so found. All of them are plausible candidates for the 470s, particularly Kimon, *meirakion pantapasin* in 489. As far as I know, there are at least three cases where Kimon and Megakles pieces join or come from the same pot. This suggests that at least a very fair proportion of the 490 Kimon ostraka from the deposit come from the same ostracism as those of Megakles. I do not think it probable that Kimon was voted against in quantity in 486, still less, that if he was, he should have failed to appear in the closed Agora deposits, and similar but weaker arguments apply to Mnesiphilos, Leagros and Kallias Hipponikou.

Further, if the Megakles ostraka belong to 486, they must carry with them a fair proportion of the whole deposit, sufficient to carry with them what has been described to me as a 'reasonable cross-section of the ostracism of 486'. However, despite the enormous scatter-vote which argues against very much organisation and concentration in this ostracism, we only find in the deposit 6 ostraka for Xanthippos, prominent at least since 489 and ostracised only two years later, 2 each for Kallixenos and Hippokrates Alkmeonidou, heavily represented in the Agora deposits agreed to belong to 483 or 482, and none for Habron, who has 4 in the much smaller Agora deposits. If we have a reasonable cross-section of 486, we will have to assume that the rise in all these people's unpopularity in the next few years was very sharp indeed.

I therefore suggest an alternative hypothesis to that of Willemsen, that the deposit is a unit and that it contains the bulk of the votes cast at an ostracism in the 470s at a time when several people under suspicion in the late 480s were no longer of much importance and when new figures, notably Kimon, Kallias Kratiou, Menon and Leagros had come to the front. It would be premature to explore the politics in detail. I merely note that the phenomenon noted by Mattingly, that Megakles is not attacked for Medism, would perhaps be less surprising at a time when he had cleared himself by returning from exile in 480. (Incidentally, I question the view that Kallias Kratiou is being so attacked. If he is described as a Mede, it may be a nickname arising out of some unknown episode; cf. Hipponikos Ammon, Lykourgos the Egyptian, Herakleides the *Basileus*.)

I have said that the hypothesis has difficulties of its own and I list them. Firstly, Thomsen reports that the deposit contains an ostrakon of Hipparchos, ostracised in 487. Lycurgus (*Leocr.* 117) says that Hipparchos son of Timarchos was condemned to || death in absence for treason, and, despite the false

patronymic, this is certainly *prima facie* evidence that Hipparchos never returned to Athens. I have not been able to discover what precisely is on this ostrakon or how its find-spot relates to that of the remainder of the deposit. If it turns out to pass on both counts, a single ostrakon may nevertheless be a stray or attributable to a mistake or aberration. Secondly, the deposit contained 36 sherds of Hippokrates Anaxileo (one of these is now reported to join 1 Themistokles and 2 Megakles ostraka) and 3 of Boutalion. The evidence of the Rectangular Rock-cut Shaft (Vanderpool, *Hesperia* 15 (1946), 266–8, summarised by Thomsen, 86–8, Meiggs and Lewis, *Greek Historical Inscriptions* pp. 43–4) is that they were 'candidates' relatively early in the 480s. Nevertheless, they are absent from the closed Agora deposits of 483/2, a fact which once led Vanderpool (*Hesperia* 21 (1952), 8) to suggest that one or the other of them was ostracised in 485. I find their appearance in the deposit uncomfortable for my theory, but supporters of Willemsen are committed to finding the same gap in the records of Kimon, Mnesiphilos, Leagros and Kallias Hipponikou unsurprising. Finally, there is the question of absolute date. Mattingly (p. 285) reports that one of the Menon ostraka refers to his archonship, and this seems to be the most likely explanation of what stands on the sherd. A Menon was archon in 473/2, and, since the archonship is referred to in the aorist, the ostracism of 471 would seem to be its earliest possible date. If this constitutes a *terminus ante quem non* for the whole deposit, serious difficulties are occasioned by the 6 ostraka reported by Thomsen for Xanthippos Ariphronos, if they belong to him and not to his cousin (?) Xanthippos Hippokratous (1 reported ostrakon). For we hear nothing of Xanthippos Ariphronos after 479, and it has been universally held that he must have been dead at the time his son Perikles was choregos for tragedy at the Dionysia of spring 472.

These difficulties combine against my alternative hypothesis, and I am very far from claiming certainty for it. We must suspend judgement, but I think that the suspension of judgement should be universal. It is being freely stated that it is now proved that Themistokles and Kimon were 'candidates' in 486, and that we know that the missing victim of 485 was Kallias Kratiou. These propositions seem far from certain to me.

14

Megakles and Eretria

I am naturally delighted that Franz Willemsen (*AM* 106 (1991), 144) has announced his adhesion to the view that Megakles (IV) son of Hippokrates (I) of Alopeke was ostracised twice and that the major part of the find of Kerameikos ostraka belongs to the 470s and not to the 480s; this was argued by me (*ZPE* 14 (1974), 1–4[1]) and Peter Bicknell (*AC* 44 (1975), 172–5). This is apparently not the end of the matter, for, on 4 December 1992, at the American School of Classical Studies at Athens conference on The Archaeology of Democracy, Stefan Brenne exhibited ostraka of Kallias Kratiou Alopekethen physically joining ostraka of Megakles and others from the find. The 718 ostraka of Kallias must also move to the 470s.

Yet another argument for this date can now be added, from the new list of Kerameikos ostraka provided by Willemsen and Brenne. The three hitherto unreported ostraka for Ἀρίφρον Χσανθίπ⟨π⟩ο (*AM* 106 (1991), 150) can only make sense after the death of Xanthippos some time after 479. That Ariphron was Xanthippos' older son was argued by J. K. Davies (*Athenian Propertied Families* 456); it is now clear that at least three voters thought that he, and not Perikles, would be the political heir of Xanthippos.

Much now needs to be done to elucidate Athenian politics of the 470s, but I confine myself here to a point of detail. It concerns an ostrakon, Kerameikos 3469, which Willemsen publishes to support the case for a second ostracism of Megakles (*AM* 106 (1991), 144–5 with Taf. 26.3). His text is

]ακλες | [ ]οκρατος | [πα]λι εχσο | εισελθεις:
ΜΕρετρ⟨ι⟩αζε;

* Published in *ZPE* 96 (1993), 51–2. Lewis's and other interpretations of this ostrakon are chronicled in *SEG* xli 16(d).

[1] See pp. 110–13, above.

his paraphrase 'Megakles ist zur Wiederholen des Weges, auf dem er hereingekommen, weg- und (aus der Stadt, aus Athen) hinausgewünscht: nach Eretria, fraglos seinem Ausgangspunkt.'

That this is an ostrakon for [Μεγ]ακλῆς | [ἱππ]οκράτος no one will doubt. As will be seen from the photograph, there are two associated inscriptions; as Angelos Matthaiou points out to me, it is not clear how many hands are involved. The two circles of separation followed by ΜΕρετρ⟨ι⟩αζε lie to the right of [Μεγ]ακλῆς; the rest of the text is aligned under the name and patronymic.

Provisionally, I accept [πά]λι(ν) ἔχσο, since the *lambda* is virtually certain; it is a worry that I see nothing really comparable in Threatte, *Grammar of Attic Inscriptions* i. 636–7 for *ny* omitted before a vowel. I cannot, however, believe in a direct combination of ἔχσο with εἰσέλθεις, and the spacing shows that something is missing. Read ||

[πά]λι(ν) ἔχσο·
[μὲ] εἰσέλθεις.

We now come to the differently incised ΜΕρετρ⟨ι⟩αζε. It has long been known that one of these ostraka was supposed to have 'Ερετρ⟨ί⟩αζε on it; I have alluded to it myself (*ap.* Burn, *Persia and the Greeks* (1984 edition) 605). I had not taken the point that it would have to be a false form based on 'Αθήναζε for the more correct 'Ερετρίανδε, but this might not be too much of a problem; there are equally difficult forms, Βήσαζε and Μουνιχιάζε, for which see the index to *The Athenian Agora*, xix. What had not emerged in oral report is that the word is preceded by a perfectly clear *my*, unexplained by Willemsen. We are therefore not dealing with 'Ερετρ⟨ί⟩αζε, but with μὲ 'ρετρίαζε, a negative imperative. I leave to others the question of whether this is prodelision or, as preferred by Threatte (*Grammar of Attic Inscriptions* i.426, 431), crasis.

Hesychius gives us ἐρετριάζει· σκώπτει. ἢ παίζει. We do not know what text he is alluding to, and we need not be bound by his semantic range for the word. I have no idea what the allusion is; it could be political; it might even be sexual. I accept that [πά]λι(ν) probably is an allusion to the fact that this is a second ostracism, but I now very much doubt that Megakles is being told to go to Eretria.

15

The Athenian Coinage Decree

The organisers have marked the importance of the Athenian Coinage Decree for our subject by calling for two papers on it. I detect a suggestion that they hope for some degree of confrontation and that Lewis in 1986 is expected to hold the same views as Meiggs and Lewis in 1969. I shall say at once that I have no confidence that I know the truth about the problems and am merely trying to look at those facts with which I think I have some competence as straightforwardly as I can. I intend, if I can, to pretend that I know nothing about the coins.

In the *Birds* of Aristophanes, produced in spring 414, a Decree-Seller offers the inhabitants of Cloud-Cuckooland a clause providing that they should use the same measures, weights and decrees as the Olophyxioi. What their relevance is no one knows, but the literal-minded, starting with Bergk,[1] had long hankered after changing the decrees (ψηφίσμασι) into coins (νομίσμασι). Amending what may be a joke is never safe procedure, but the joke has to have some foundation, and Wilamowitz in 1877[2] suggested that there might have been an Athenian law enforcing uniformity of coinage, weights and measures in the empire. He tells us that he was laughed at for the idea, but, totally unrecognised by anybody, a substantial fragment of such a law was already known, copied at Smyrna in 1855; no one has seen it since, and doubtless it perished in the burning of Smyrna in 1922.

* Published in Carradice (ed.), *Coinage and Administration in the Athenian and Persian Empires* (1987), 53–63. H. B. Mattingly's paper was published on pp. 65–71. Discussion of the decree continues; in *CAH* v² 130–1, Lewis inclines to an early date; at *IG* i³ 1453 he re-edits the fragments.

[1] In his Teubner edition of 1857, but I am not sure that he meant that the text was faulty; Blaydes and Van Leeuwen made the emendation.

[2] U. von Wilamowitz-Moellendorff, *Aus Kydathen* 30, *Reden und Vorträge*³ 52.

It was not until 1894, when a second fragment turned up on Siphnos, that Adolf Wilhelm confirmed Wilamowitz's suggestion, and even then no full publication followed until 1903.

It was on the ground of this publication that discussion began and that R. Weil[3] and Percy Gardner[4] started to contemplate the numismatic record in the light of the epigraphic text. They were fairly clear that at least two decrees were involved, since the text appeared to refer to 'the previous decree of Klearchos' (τὸ πρότε]ρον ψήφισμα ὃ Κλέαρχ[ος εἶπεν). They agreed that these decrees were late and that one of them at least was news at the time of the *Birds*, though their general impression of the coinage was that Athens had in fact tried to impose uniformity since the middle of the century. The epigraphists were not altogether || happy with a late date for the decree, and wanted to move it back at least a little to accommodate the more archaic form of the dative feminine plural, which appeared in the text and was held to disappear around 420; the epigraphic consensus tended towards putting the first decree early in the Peloponnesian War and the surviving texts around 422. Relatively little was added to the argument, as it stood then, by the recognition in 1924 of two fragments from Syme first published in 1922.

No question of epigraphic lettering had so far entered the argument. All the copies were in the Ionic alphabet, and did not seem to be at all closely datable. The situation changed in the late 1930s. A large fragment from Aphytis, in a competent, though rather anonymous, Ionic hand, was published in 1935, but very little progress had been made in assessing its contribution to the text when it was overtaken by a much more sensational discovery, a large fragment from Cos, first published in 1938. Unlike all the other copies, it was in Attic script, and Segre, who published it,[5] thought it was on Pentelic marble. Segre pointed out that the decree ordered each ally to publish it on stone and appeared to say that the Athenians would do it if they didn't. The Cos fragment was, he thought, imported from Athens and could be judged stylistically as an Attic text. It was therefore of the utmost significance that it used the three-bar *sigma*, of which there is no dated Athenian example after 446. By fairly general consensus, the whole decree was then moved to the early 440s and seen as a manifestation of Athenian imperialism in the period immediately after the Peace of Kallias. Stanley

[3] R. Weil, *ZfN* 25 (1906), 52–62, 28 (1910), 351–64.
[4] P. Gardner, *JHS* 33 (1913), 147–88. [5] M. Segre, *Cl. Rh.* 9 (1938), 151–78.

Robinson,[6] at least, saw very little difficulty in this from the numismatic point of view. Since we have already seen Weil and Gardner operating in the same direction, it would not be fair, even if we were talking of someone of lesser stature, to say that the numismatist was falling into line with the epigraphic orthodoxy.

The Cos fragment has not marked the end of new acquisitions. In 1933 the Odessa Archaeological Museum acquired a tiny fragment which it unloaded on to Odessa University. No one seems to have thought much of it, and, although its text was published in 1959 and it was recognised as a fragment of the Coinage Decree in 1960, the university seems to have lost it. There can be no certainty about its provenance. The only positive evidence is for Olbia, but this has been disputed, and, if I read the description of the stone, in Ukrainian, accurately, it was on finely granulated bluish marble, which doesn't seem normal for Olbia.

In 1961, Harold Mattingly opened his counter-attack,[7] and so we are celebrating, not merely the new hoard, but also the silver jubilee of the first of a long series of ingenious articles. Of course, although the first article was called 'The Athenian Coinage Decree', study of this decree is only one facet of a much broader approach to the Athenian empire on two levels. There is the technical level, in which challenge to the dogma about the date of three-bar *sigma* has been strengthened by careful investigations of epigraphic language and forms, and the historical level, which, roughly speaking, tends to the demonstration that much of what we think of as Athenian imperialism was a creation of the period of the || Peloponnesian War and not of an earlier period. I am not sure that the arguments, on this level, have cumulative force, and I think I could find myself accepting a late date for, say, the Coinage Decree without changing my mind very much about the imperialism I see in the late 450s and early 440s.

Let me remind you briefly of what we have,[8] on the assumption that our fragments all go together to make a continuous text. Nothing useful survives of clause 1. Clause 2, very fragmentary, seems to provide that the *hellenotamiai* shall write up names of cities; if they don't do this correctly, they will

[6] E. S. G. Robinson, *Hesperia* Supp. 8 (1949), 324–40.

[7] H. B. Mattingly, *Historia* 10 (1961), 148–88.

[8] For the text and its history, see *Athenian Tribute Lists* ii. 61–8 D14, ML 45, E. Erxleben, *APF* 19 (1969), 91–139, 212 (with commentary on the coins and the dating, *ibid.* 20 (1970), 66–132, 21 (1971), 145–62).

be prosecuted. 3 provides that if any of the magistrates in the cities, citizen or foreigner, does not act according to what has been voted, he shall suffer heavy penalties. 4 provides that, if there are no resident Athenian magistrates, the local magistrates shall act. 5 would be crucial, but is in the worst condition. It is generally assumed that it describes what is to happen in the mint; references to 'not less than half', '*x* drachmae in the mina', and to 'changing' are all that is preserved, though some quite elegant Greek has been filled in in the gaps. There have been recent changes[9] in the interpretation of what I would now call 6, covering what has recently been 6, 7 and 8. To my mind, what is being said is that the surplus from the minting operations is to go into a special fund, which may have something to do with Hephaistos, and that any attempt or proposal to use it for anything else makes the offender liable to the death penalty. I am sure that the traditional interpretation, which makes proposals for the use of foreign money liable to the death penalty, is wrong. 7 (old 9) provides for the appointment of four heralds to announce what has been voted. One is to go to the Islands, one to the Hellespont, one to the Thraceward area; presumably the fourth is to go to Ionia. More was said about the method of their despatch, but we have lost a good deal. 8 (old 10) is the publication clause we have already referred to. 9 (old 11) seems to be an afterthought about what the heralds are to say. 10 (old 12) is fortunately very well preserved. The secretary of the Athenian council is to add the following to the bouleutic oath: 'If anyone mints silver coin in the cities and does not use Athenian coins or weights or measures but [foreign coins], weights and measures, [I shall punish him and fine him according to the previous] decree which Klearchos [proposed].' 11 (old 13) is quite hopeless, and about 12 we can really only say that the *epistatai* of the mint are to publish lists of something, for anyone who wishes to look at them. How far the text extended on either side of all this is anybody's guess; the small fragment from Syme could take us into ground otherwise not covered, and Mattingly has considered adding a large fragment of related subject-matter from Athens itself (*IG* i^3 90).

How do the epigraphic and historical arguments about the date stand? First, the find-spots. Smyrna was never a tribute-paying member of the empire, and it is in any case clear that the fragment formed part of a collection

[9] D. M. Lewis, in Φόρος: *Tribute to Benjamin Dean Meritt* 83–5 = this volume, 139–42; H. B. Mattingly, *AJP* 95 (1974), 280–5; cf. R. S. Stroud, *CSCA* 7 (1974), 280–2.

assembled from various || places. Odessa is also a place to which wandering stones have come, and the rather confused evidence that its fragment may have come from Olbia is matched not only by my doubts about the marble but by the absence of any other evidence that Olbia was ever subject to Athens; it will hardly do much to prove that the Decree postdates 425, the first year in which we know that Athens claimed tribute from the Black Sea area. Aphytis, paying tribute from at least 451, presents no problem. Siphnos starts appearing in the tribute lists from 449; it is to my mind overwhelmingly probable that this is her first appearance, whether because she had been contributing ships up to this point or because she had been through a period of disaffection.

As far as the Cos fragment is concerned, there is an initial doubt, since it was found in the modern city, which, on all reasonable evidence, was not the state centre of Cos until 366. Segre challenged that date, but there are preferable solutions. Cook and Bean[10] came down for the hypothesis that there was an Athenian naval base, at which this Attic copy was set up, but allowed the possibility that the stone had come from somewhere else. If that is so, the stone may not have been originally set up in Cos at all. Cos first appears in the tribute lists in 450. Again, it is unlikely that she had paid tribute earlier. Her tribute record for the next four years is chaotic, with many partial payments, and for 445 to 443 she is not attested at all. This provides an admirable context for us to set a city so uncooperative that she had to have a copy of the decree set up for her, but of course this is no more than suggestive; she has an unexplained short payment in 431 as well.

Finally, I come to Syme. It does not appear in the tribute lists until 433, and, when it does, it appears in a way which more or less proves it is a new arrival, under the heading of cities inscribed to pay by individuals, that is, on the best view,[11] by a party in Syme who were prepared to pay 1,800 dr. a year to establish some claim to Athenian protection. It only gradually acquires a less anomalous status in the early years of the Peloponnesian War. Does this prove that 433 is the earliest possible date for the Coinage Decree?[12] One

[10] G. E. Bean and J. M. Cook, *BSA* 52 (1957), 124–5.

[11] W. Schuller, *ZPE* 42 (1981), 141–51.

[12] I now withdraw my early suggestion ('Towards a Historian's Text of Thucydides', 44–6) that these Συμαῖοι were not from our Syme at all, but represented the fifth-century name, appearing in Diodorus, of the Thracian city of Oisyme; Martin Price has obtained for me a photograph of the unique coin of Oisyme in Paris, and the full name is clear there.

could weaken the force of the *terminus post quem* by saying that Syme might have up to that time paid through a larger group, and that she was in the empire enough to feel she had to exhibit the Decree. The alternative escape route is to suppose that a new member of the empire was required on its accession to exhibit a copy of the pre-existing Decree. These are possibilities, but clearly the argument that the Syme fragments prove a date after 433 is very strong and deserves a more prominent place in the argument than it generally gets; Mattingly did not produce it until 1966;[13] it is not mentioned in Meiggs and Lewis, Erxleben, Meiggs[14] or Schuller.[15] ||

Next, the letter forms. All the fragments except for Cos are in Ionic. I must say, as a preliminary, that epigraphists are extraordinarily bad at dating classical Ionic texts and that the wildest differences of opinion are current. This may go back to a time when everybody's views were coloured by the belief that there were no Ionic inscriptions at Athens before 403/2, and there can still be considerable disagreement and muddle even as to whether a text is fifth- or fourth-century. This can happen even at Athens, where there is plentiful dated material. There has just been the extraordinarily embarrassing case of the Thorikos Calendar in Malibu, which Georges Daux has dated to 385–370, overlooking, I may say, one of those early feminine dative plurals. About this, I am only prepared to repeat what I have said in print,[16] that, had it not been for the authority of the editor, I would have unhesitatingly ascribed it to the decade 440–430. And, once one gets outside Athens, precisely dated material hardly exists anywhere, certainly not at Aphytis, Siphnos, or Syme. In the circumstances, it is folly, though it has been done, to worry at all about the fact that three Russians, inexperienced as epigraphists, two of whom do not appear even to have seen the stone, thought that the Odessa fragment looked fourth-century. No real argument, over the period 450–413, is going to come out of looking at the Ionic fragments, two of which are lost and two only available in terrible photographs. Smyrna was said to have shown the 'beautiful simple traits of the Attic period', Siphnos was certainly a fairly sloppy job, Syme not much better. Aphytis alone exhibits a decently professional, though rather anonymous, hand; if it had been an Athenian text, only some tendency to angularity in the rhos would cast

[13] H. B. Mattingly, in *Ancient Society and Institutions . . . Ehrenberg* 195–6.
[14] Meiggs, *The Athenian Empire* 167–71.
[15] Schuller, *Die Herrschaft der Athener im Ersten Attischen Seebund* 211–17.
[16] D. M. Lewis, *ZPE* 60 (1985), 108 n. 3.

doubt on the dating 430–410, and, to go outside Athens, it looks a shade later than a Milesian text which probably belongs to 435. If there were any justification for matching the texts against each other, it is the Syme text which one might have said looked earlier than Siphnos or Aphytis. All this is impressionistic; we cannot say that Ionic lettering moved at the same pace everywhere.

I now turn to the Cos fragment. Let me say at once that, after the work of Meiggs[17] and Walbank[18] stimulated by Mattingly's assault, I remain more than ever convinced of the essential correctness of conventional Attic epigraphic dating. I haven't yet discussed it with him, but one of his better cases has just taken a hard knock. Just after the best demonstration he has yet given[19] that the first decree for the priestess of Athena Nike (ML 44) should belong to the 420s and not the early 440s, it has been shown[20] that its stonecutter was the same man who cut the accounts for Pheidias' statue of Athena Promachos. I don't know whether he will argue that the Promachos is later than we had supposed or that the accounts are for something else or that a fifth-century Athenian stonecutter might be recognisable over a twenty-five year period. I remain convinced that, for public inscriptions, any attempt to date a well-attested letter form like three-bar *sigma* outside its attested range starts with a serious handicap. ||

All this could of course be described as a malicious digression, only tangentially relevant to the Cos fragment. Segre's original observation, that the marble was Pentelic and the stone was therefore imported already carved from Athens, cannot survive the petrographic analysis given by Georgiadis.[21] The marble was not Pentelic, although inspection of Georgiadis' data by Jack Zussman, Professor of Geology at Manchester, confirms my suspicion that he has not shown that it was Parian, merely that it was more like Parian than any of the other samples scrutinized in that particular article. Parian marble was of course available at Athens, but it is not likely that it would be thought economical to use it. If the stone was not carved at Athens, it can be and has been maintained[22] that the script, Attic though it is, need not

[17] R. Meiggs, *JHS* 86 (1966), 86–98.

[18] M. B. Walbank, in Φόρος (see n. 9), 161–9 (= id., *Athenian Proxenies of the Fifth Century BC* 31–51).

[19] H. B. Mattingly, *AJA* 86 (1982), 381–5.

[20] S. V. Tracy, in *Studies Presented to Sterling Dow* 281–2.

[21] A. Georgiadis, *BCH* 89 (1965), 400–22. [22] W. K. Pritchett, *ibid.* 425.

be judged in the same way as one would judge a text from Athens. On this, Meiggs and Lewis commented: 'But why should a mason, whether Coan or Athenian, working in Cos use an Attic sigma that had been obsolete for more than twenty years, when none of his other letters suggests that he was old-fashioned? The epigraphic argument may be a little weakened, but it remains strong.'

I can think of various possible answers to our question, notably one arising from the fact that Athens had, for a considerable time, been virtually unique in the Attic-Ionian world in using three-bar *sigma*; even Cos, not technically part of that world, had the fourth bar. Even some time after Athens had abandoned it, a foreigner might think that he was giving the proper flavour by using it; the Spartan text from Delos of 402–400 (Tod 99) is very unlike anything we know of from contemporary Laconia. I can only say that our text does not strike me as the work of a non-Athenian or even an amateur. To put the matter in its context, it is worth looking at some other Athenian public or semi-public texts carved or exhibited abroad in the fifth century. There are two other decrees, both in Ionic. I have not seen the Delos text (*ZPE* 60 (1985), 108); presumably its script was like the fifth-century accounts from Delos, which are straightforwardly Ionic and were surely carved there. Squeezes of a lost decree from Karpathos (Tod 110), long thought to be fourth-century, have now turned up in Berlin; it turns out, Ionic though it is, to be carved by a respectable Athenian professional, probably in the 430s; taking the script together with the fact that an epigraphist with far broader experience than Segre thought the stone Pentelic suggests that we have one case here of an Athenian text being shipped a very long way. Of the two inventories drawn up by Athenian cleruchs on Aegina, one (*IG* iv 1588) is an amateurish mess in mixed script and the other (*IG* iv 39) fairly straightforwardly Ionic. There are public dedications from Delphi (*Fouilles de Delphes* iii. 4 190) and Dodona (*SIG*^{3} 73) which use four-bar *sigma* unexpectedly early. Only a group of rather mysterious *leges sacrae* from mid-century Delphi (Sokolowski, *Lois sacrées des cités grecques, Supplément* 40) are anything like pure Attic of their period. All this suggests to me that, if whoever was responsible for putting up the Cos text had to get it done locally, he was unusual in stipulating for an Attic text and fortunate to find someone who was capable of turning out so good an imitation of an || Athenian text. It cannot of course be determined whether this cutter was still in the main stream.

I shall say little about the other dating points which have been adduced. The argument about the long plural datives still holds up more or less. It may be interesting to observe that there are two in texts which almost certainly belong to the Athenian recodification of the laws in 410–404 (*IG* i^3 236.37, 237 *bis*.4); I have attributed that to their appearance in the originals which were being recopied. On the extremely copious literature on the heralds' journeys, I propose to say next to nothing. The argument was originally tied up with Pericles' Congress Decree, which is no longer a respectable text; I have never seen any necessary connection between the journeys and the way that tribute lists and assessments are organised; I am perfectly prepared to contemplate the possibility that the Athenians varied their procedures and word-order without there being any particular significance to the variations.

Before I come on to more general matters, there is one tiresome point to mention, the 'previous' decree of Klearchos. The restoration depends only on Baumeister's copy of the Smyrna fragment; he read ρον. πρότε]ρον goes back to Wilhelm; δεύτε]ρον and ὕστε]ρον, suggested by Segre, would not alter the implication that Klearchos proposed two decrees. The authors of *ATL* denied that implication, saying,[23] not very convincingly: 'There was, we hold, no other such decree, for the provisions and penalties which each councillor swore to enforce must have been those of the present text. It is just possible that Klearchos, in phrasing the oath, called his own decree πρότερον, because for the future swearer the decree would be "earlier" than his oath.' They did not really like that themselves, perhaps wrongly, as we shall see, and continued 'but we note the possibility that the letter read by Baumeister as Π was really Ν; this would allow the restoration [γενόμε]νον'; the advantage of that restoration would seem to be that it means nothing. I would also reject the other attempt to break away from the two decrees. Erxleben suggests ἡμέτε]ρον, 'our', but I doubt whether the Athenian Council would describe a decree of the Athenian Assembly as 'our decree'.

I will leave it to Mattingly to expound his ideas about *IG* i^3 90, which may or may not be relevant, but, of course, if Klearchos moved two decrees about coinage and related matters, most of our effort in trying to date our fragments has been a waste of time. It would remain perfectly possible to hypothesise an earlier attempt to enact these or similar provisions and date

[23] *ATL* ii. 67.

this whenever your view of the coins suggests. Empires, as a short look at the Roman *Digest* will show, sometimes have to repeat themselves when measures do not work, and the Athenians, who certainly made more than one attempt to regularise the delivery of tribute, can have made more than one to tidy up this question. I have in the past considered distributing our fragments between two decrees. If the second decree incorporated large parts of the first, it would, I think, be just possible to keep Cos in the early 440s, to keep us orthodox epigraphists happy, and make everything else later, but I am not recommending this line very strongly. ||

There is also the other possibility, suggested by Tod,[24] that 'Klearchos' earlier decree' was not about these matters at all, but about legal procedures to be followed in imperial offences. If you are used to looking at combined texts, you may think that that would be a rather cryptic reference, but there probably was room on the Smyrna fragment to add a word or two to define the content of the decree, and that would certainly make things easier. Compare the closest parallel, *IG* i^3 107.6–8:

καὶ τ]ἄλλα α[ὐτ]οῖν ἔναι κατὰ [τὸ πρότερον ψήφ]-
[ισμα ὃ . . . ιπ]ος ε[ἶ]πε περὶ τῶν εὐεργ[ετῶν τῶ δήμο τ]-
[ῶν πρότερον] ἐξεληλυθότων ἐκ τῶν πό[λεων.

It is maddening that πρότερον is even more restored here than it is in our text, since it looks as if it would provide a much needed parallel for *ATL*'s suggestion that it need mean no more than 'previous to the present time'. Perhaps the evidence for there having been two Coinage Decrees is not all that strong.

It is more than time to assess the general motivation of the Decree. Wilamowitz, who started it all, was giving a ceremonial lecture on the Athenian Empire to celebrate the Kaiser's birthday in 1877, and saw the Athenian empire as an enlightened forerunner of the new German Empire. In making the suggestion that there was a Coinage Decree, he saw Athens as stimulating general economic progress, as well as profiting herself from the gain involved in reminting a multitude of strange currencies into her own. The two strands of this have had numerous successors. On the one side, some have almost thought of the Decree as part of Athens' attempt to create a sort of free trade area. On the other, there has always been a school

[24] Tod i, p. 166.

of thought with a keen eye on the profits to be made by reminting and in other ways; there has been a very recent article[25] arguing that hardheaded calculations about economic advantage always played an essential role in Greek coinage. Both strands, of course, tie in to the major division among modern scholars about ancient economics, that between modernists and primitivists. Those who hold that the ancients did not necessarily think about economic matters in modern capitalist ways have poured scorn on the idea that the main motive to the Coinage Decree can have been anything which we would call either macro- or micro-economic. Moses Finley, of course, has been the main exponent of this view, and we can consider one of his statements of it in full:[26]

> Equally political was the fifth-century BC Athenian decree which laid down the rule that Athenian coins alone were to be current for all purposes within the Athenian empire . . . The political element is unmistakable: the unprecedented volume of Athenian military and administrative payments, at a time when foreign tribute was the largest source of public revenue, was much facilitated by a uniform coinage, and Athens was now able and willing to demonstrate who was master within the empire by denying the subject-states the traditional symbol of autonomy, their own coins. The Athenians may || also have aimed at mint profits, but we shall not know until the missing bit of the text stating the mint charge for re-coining is found.
>
> It is also held that there was a commercial motive, a desire to give Athenian merchants the advantage over others.[27] The logic escapes me. Everyone had been equally the victim of a profusion of mints; had the Athenians been able to enforce their decree for a sufficient number of years, everyone within the empire would have benefited slightly but equally, the Athenians no more than the others, questions of pride and patriotism apart. Only the money-changers would have been the losers, and no one has yet suggested that such a powerful decree was passed just to hurt them.

I do not think that anyone seriously holds now, if they ever did, that the Athenians were trying to benefit their merchants,[28] but the tide certainly

[25] H. Engelmann, *ZPE* 60 (1985), 165–76.

[26] M. I. Finley, *The Ancient Economy*, 168–9; see also *2nd International Conference of Economic History* i. 22–5.

[27] I think the allusion may be to C. H. V. Sutherland, *AJP* 64 (1943), 145 with n. 3, which does not mention the Decree at all.

[28] Meiggs, *The Athenian Empire* 172, rather vaguely, comes close to it.

seems to be turning against this fairly pure political explanation. The political explanation, of course, does not explain the apparent exclusion of electrum; one could only suggest that the value of electrum coinages to Athens in the Black Sea trade forced their exclusion. What is far more serious is that the political explanation, however it might work for coins (and Robinson too[29] speaks of the mass of Aeginetan money still circulating being a continual reminder of Aegina's former greatness and a continual irritation to Athens), becomes progressively weaker when applied to the weights and measures, which are after all in the decree, though they may not interest numismatists very much.

There is surely much in what Finley himself says about the unprecedented volume of military and administrative payments being much facilitated by a uniform coinage. The point was taken up by Starr,[30] who found reason to suppose that Athens recoined the treasury of the Delian League when it arrived in Athens, by Will[31] and by Schuller.[32] The great advantage of this line of thinking, it seems to me, is that it will readily accommodate the weights and measures as well. I had written a short piece along these lines for *CAH* v², but I have now been overtaken by Thomas R. Martin.[33] Martin's main concern, of course, is to debunk the link between coinage and sovereignty, but he has a good treatment of this point too. I am not suggesting that anyone responsible for Athenian legislation knew or cared what length of foot the builders used on Samos, to take our well-known local example. Martin concentrates on the revenue aspect of the measures, the need to have an agreed standard for taxing the imports of, say, Thasian wine. I am more concerned with the needs of Athens' administrative and military machine. Twice in the early years of the Peloponnesian War (*IG* i³ 61.34–41, 62.1–5), states are given the privilege of drawing *x* thousand *medimnoi* of corn directly from Byzantium. The type of *medimnoi* does not need to be stated (contrast Tod 140.80ff., where the Delphic *naopoioi* have to convert from Pheidoneian to Delphic *medimnoi* before putting a cash value on a gift of barley); something must have been done to make sure that everyone knew that they were talking about the || same measure. And what about the needs of the Athenian fleet, by far the greatest economic complex of ancient Greece? Quite apart from the imports for original construction, it must constantly

[29] *Hesperia* Supp. 8 (1949), 325. [30] Starr, *Athenian Coinage 480–449 BC* 69f.
[31] E. Will, *Le Monde grec et l'orient* i. 207f., 209f. [32] *Die Herrschaft* (n. 15), 216.
[33] Martin, *Sovereignty and Coinage in Classical Greece* 203–4.

have been buying supplies of all kinds of commodities, from nails to paint, all over the place. Would it not make life a great deal easier if some kind of standards could be arrived at, so that haggling could be done about mutually understood quantities? That it would make life a great deal easier all round if coinage was uniform and of clear value is of course common ground, though surely our emphasis should be on state, not private, transactions.

If we continue to think along these lines, there are perhaps a few more chronological pointers to be gleaned by looking around. We have already seen that *medimnoi* of corn needed no definition by 426. Weights of bronze, certainly, and perhaps tin were qualified in some way in the 450s (*IG* i^3 435.69–71, 101–5), not in the years after 421 (*IG* i^3 472.139ff.). That does not get us far. Let us return to coinage.

It is well known that electrum was, at any rate originally, an acceptable way of paying Athenian tribute. The first Athenian list, of 453, had a postscript which divided Athena's sixtieth into silver and Cyzicene staters (*IG* i^3 259. postscript 6–13), though the total tribute paid in Cyzicenes cannot have been even as much as 10 per cent of the whole and was probably a good deal less. The lists contain various odd amounts of tribute. Some of them appear to be official, that is to say, with tributes actually assessed in terms of electrum and showing a fair amount of consistency. Others do not. For many years I kept notes on these, wondering whether the odd sums recorded might represent situations where the allied state was tendering coinage in the belief that it had a particular value, but the Athenian *hellenotamiai* were only prepared to tariff them at a lower value. I made periodic attempts to write them up, but repeatedly came to the conclusion that my arithmetic was getting fancier and fancier, that I could not myself draw the line between plausible and implausible cases, and that other people were likely to draw the line far less charitably. But an article was needed and was eventually produced by Eddy in 1973.[34] It is certainly vulnerable in some of the ways which I feared, but it is nevertheless interesting reading. The most solid result seems to be that the irregular amounts more or less disappear between 446 and 430, only starting up again during the Peloponnesian War. Eddy suggests that this was the period when Athens really was insisting on uniformity of coinage, brought it into line with Robinson's chronology for allied coinages, and arrived at a date for the Coinage Decree. 'If we apply the

[34] S. K. Eddy, *AJP* 94 (1973), 47–70.

evidence in the quota lists strictly, and if we can believe that the Athenian ban on minting silver coins discouraged the use of electrum, then we might date the decree exactly to 446 BC.'

As stated, this is not likely to convince any one, I fear. The Decree does not mention electrum. My suspicion, however, is that Eddy may still be on the right track, but is paying the penalty for explaining too many of his anomalous payments in electrum. Some of my fancier unpublished arithmetic involves silver coinages instead. And there is this to be said. It is not only these uncertain sums in the quota lists which suggest that Athens went through a long period of avoiding || the official use of electrum. It is a well-known fact that in 447 someone succeeded in unloading on the Parthenon commissioners 74 Lampsacene staters and 27 Cyzicenes (plus a *hekte*) and that this electrum remained unspent for the next fifteen years. If we leave out some odd bits of foreign money owned by various gods in 429/8 (*IG* i^3 383 *passim*), it is not until 418/7 that we find electrum playing any part in official Athenian accounts.[35] Of course, it can very simply be said that they will use Attic coin for convenience as long as they have it and only draw on other coinages when they do not. Eddy's line of thought, by which it is the period 446–430 in which uniformity is aimed at, could, I suppose, be matched by a supporter of a late dating for the Decree. It could be maintained that a purpose of the Decree was to restore the circulation of Attic owls and to bring back into the system supplies which had been draining from it.

I conclude by considering the principles on which the epigraphic and numismatic evidence should be brought together. As a result of the conference, I accept that there could be numismatic evidence which might date the Decree, though the apparent continuity of northern coinages points in a different direction from the unexpected appearance of electrum at Chios. What I remain very doubtful about is whether an independent dating of the Decree would or should make any serious difference to correct numismatic operations. You will have gathered that I find it helpful sometimes to go back to the history of a question, and in this case I did look to see how the great historians of the first part of the century had reacted to the discovery that Wilamowitz's suggestion had been correct. I have not yet discovered that the greatest of them all, Eduard Meyer, took any note of the matter at all, but it is, I think, well worth looking at the 1908 treatment by

[35] *IG* i^3 370.13; several instances thereafter.

Cavaignac,[36] largely independent of Weil. For Cavaignac, it was merely the position of Athens and its administrators in the Delian League after 478 which inevitably reduced the need, as well as the utility, of coinage for the allied states. The reduction and disappearance of their coinages which he saw rested, he thought, on the facts of the situation. That the Athenians eventually attempted to regulate the matter by decree was a sign of weakness, not of strength, as rival coinages, starting from the successful revolt of the Chalcidian confederacy, began to challenge the primacy of Athenian coinage. It is not totally clear to me what Cavaignac, who had laid proper emphasis on administrative considerations, meant by saying that Athens wanted to preserve her *privilège monétaire*, but I think I do want to know how a numismatist studying a particular coinage is to distinguish between a cessation of coinage imposed from outside and a simple stop because there is no need to coin. That, incidentally, is what Martin's book is about, in relation to fourth-century Thessaly. The cities, he thinks, stopped, not because Philip destroyed their autonomy, but because they could not afford to coin and had no need to. The difference in our case is that we do have evidence for external action, but I am not sure how, in the circumstances, we really expect that the Decree can do anything to date any coinage.

[36] Cavaignac, *Études sur l'histoire financière d'Athènes au V^e siècle* 177–87; Beloch, *GG*[2] ii. 1, 92 is dependent on this.

16

Athena's robe

Athens was Alexander Fuks' first love, and it is a matter of regret that he never carried out a plan to translate what he affectionately described as his 'Hebrew Zimmern'. I offer here in his memory a short note to show how much in the dark we can still be about the most central issues.

It has been generally assumed[1] that the robe (*peplos*) offered to Athena at the Great Panathenaea was placed on the olive-wood statue of immemorial antiquity, which was certainly small and portable. The view recently expressed by H. W. Parke,[2] that by the late fifth century the *peplos* was of colossal size and offered to Pheidias' chryselephantine statue of Athena, dedicated in 438 BC, has been treated as heresy by at least one reviewer, G. T. W. Hooker.[3] The matter seems to me to be more open than that.

Parke is clearly relying on fragment 30 (Kock = Edmonds = Kassel and Austin) of the *Macedonians* of the Athenian comic poet Strattis; the date is uncertain, but cannot be far from 400 BC. The translation must be something like 'This robe with ropes and windlasses countless men haul up like a sail on its mast.' Hooker comments 'We do not know the context, nor whether there is any element of comic exaggeration here; but the speaker is not saying that the *peplos* was as big as a sail, only that it was hauled up in the same way.' But the countless men are outside the comparison, and, whatever the exaggeration, it seems hard to think that many men would be required for a small *peplos*.

There has long been evidence that a mast and cross-stay were important

* Published in *Scripta Classica Israelica* 5 (1979/80), 28–9. Against the suggestion that the *peplos* was a large robe see J. H. Kroll, *Hesperia* Supp. 20 (1982), 65–76 (not referring to this article).

[1] As far as I can see, Deubner, *Attische Feste* 29–34, the fullest collection of evidence on the *peplos*, takes no position on this, but see Herington, *Athena Parthenos and Athena Polias* 25, with references.

[2] *Festivals of the Athenians*, 39. [3] *JHS* 98 (1978), 190–1.

for the *peplos* as early as 299/8 BC, when new ones were provided by King Lysimachos, in control of timber-rich Thrace.[4] Further || evidence for the importance of getting the right equipment in the early third century comes from a new inscription,[5] which describes a successful application to Ptolemy II in 282 or 278 for ropes for the *peplos*. The importance of Egypt to mainland Greece for cordage needs no demonstration.[6] That the equipment needed was on a scale to support a large robe is clear, and on the face of it the view that Strattis is describing a large *peplos* is strengthened. Since the application to Lysimachos slightly antedates the occasion in 297 or 296 when Lachares stripped the gold plates off Pheidias' statue, there is no temptation to believe that it was this stripping which occasioned a change of statues, and no other occasion for a change of statues suggests itself; a change is hardly to be attributed to the conservative Lycurgus.

Other points have been raised. Hooker's objection that the Parthenon frieze depicts a small *peplos*, estimated at 4 by 7 feet, is substantially weakened by Boardman's demonstration[7] that the frieze does not represent the contemporary festival. The passages thought by Herington[8] to show that the *peplos* was put on the olive-wood statue refer to the Plynteria, not to the Panathenaea, and the inscription he quotes is in any case earlier than 438 BC. On the other side, we can add that the *peplos* took nine months to make.[9]

I therefore conclude that there is some probability that, as soon as Pheidias' statue was completed in 438 BC, a central religious rite of the Athenian state was transferred to it. If this could be more firmly established, it would be a cardinal piece of evidence for our understanding of Periclean Athens.

[4] Deubner, 32 n. 2.

[5] T. L. Shear, *Hesperia* Supp. 17 (1978), pp. 3–4 = *SEG* xxviii 60, lines 64–70. Shear's commentary, pp. 39–44, is largely concerned with the Panathenaic ship.

[6] Her. VII.25.1, Hermippos fr. 63.12–13 Kock = Edmonds = Kassel and Austin, Diodorus XIV.79.4 (Egypt cannot be a source of wooden ship-equipment).

[7] *Festschrift für Frank Brommer* 39–49. That the *peplos* is depicted here is denied by Nagy, *CP* 73 (1978), 136–41, who accepts the evidence for a large *peplos*, but thinks that the olive-wood statue was large.

[8] *Athena Parthenos* 17 n. 2.

[9] Deubner, 31. That large numbers of *ergastinai* prepared the wool for the *peplos c.* 100 BC (*ibid.* and cf. Nagy) probably proves nothing, since these noble ladies may only have put in a fairly formal appearance.

17

The treaties with Leontini and Rhegion (Meiggs–Lewis 63–64)

Ruschenbusch attempts to strengthen the view of Smart that both prescripts were recut immediately by showing that the divergences between the earlier and later texts of both were both of thirty-six letters and caused by the same error. I cannot help believing that Smart was wiser to describe the error, if such it was, as irrecoverable. It is indeed certain that a full text of both the later prescripts would have included πρῶτος between Κριτιάδες and ἐγραμμάτευε. However, the assumption, originally made by Meritt, that, because the Leontine delegation had three ambassadors and a secretary, the Rhegine delegation would have had the same and that γραμματεὺς dropped out of the inscribed text, seems unwarranted in view of the very diverse composition of Greek embassies. Even this assumption does not bring the count right, and Ruschenbusch has to assume a lost three-letter *vacat* in Rhegion line 4, thus giving his supposed secretary the same unusually short length for a name-patronymic combination as the preceding Σιλενὸς Φόκο. The fifty-one name-patronymic combinations for foreigners in *IG* i^3 show a median length of seventeen letters and only two combinations with as few as eleven letters. It seems statistically unlikely that a third eleven-letter combination should appear precisely here. Nor do I much care for Ruschenbusch's view that the mason (surely rather the Secretary of the Council) provided his assistants with the necessary separate texts for the ambassadors and the treaties, but only one (faulty) copy between them of

* Published in *ZPE* 22 (1976), 223–5. These texts are edited by Meritt and McGregor as *IG* i^3 53–4, with dates of *c.* 448 (Rhegion) and *c.* 448 or earlier (Leontini) for the original inscriptions.

Bibliography: B. D. Meritt, *CQ* 40 (1946), 85–91; H. B. Mattingly, *Historia* 12 (1963), 272; J. D. Smart, *JHS* 92 (1972), 144–6; E. Ruschenbusch, *ZPE* 19 (1975), 225–32.

Photographs: Rhegion: Austin, *The Stoichedon Style in Greek Inscriptions*, pl. 6; Leontini: Bradeen and McGregor, *Studies in Fifth Century Attic Epigraphy*, pl. 20.

the prescript which cannot have been more than a quarter of the whole text. Those who can believe this next have to believe that the conscientious *diorthotes* who had observed the original thirty-six-letter error went off duty before observing that the revised Leontini text had lost the word πρῶτος and the revised Rhegion text the word γραμματεύς. Meritt in fact thinks the omission of πρῶτος was intentional (p. 86).

There is a basic objection to calculations of the type engaged in by Ruschenbusch. He || has in fact misunderstood the nature of the correction, as he shows by marking only Leontini lines 2–15 and Rhegion lines 2–8 as 'in Rasur'. In both cases the whole surface has been removed from the very top of the stone downwards. No 'extra line' has been taken into use; the space required for the new texts has been reduced by narrowing the vertical chequer, from 0.022 to 0.0176 in the case of Rhegion, from 0.023 to 0.021 in the case of Leontini.

We have all cited Meritt for the demonstration that the deleted part of the Rhegion text contained seven lines and the deleted part of Leontini fourteen. As either calculation or measurement on the photograph will show those who have no access to stone or squeezes, this is probably an overestimate in the case of Rhegion, since it will bring the top of the old line 1 right up against the top of the stone, even closer than the present line 1. This observation shows that both Meritt's figures are maxima. We have no means of saying how far down the stone the previous texts began; we cannot even exclude the possibility that they had headings which were quite differently spaced. It is in reality quite wrong to suppose that we can form a clear idea of the length or shape of the earlier texts, still less attempt any calculation of the difference between them and their replacements.

It should perhaps be made clearer what is involved in the view of Mattingly, Smart and Ruschenbusch that both original texts were composed and inscribed in 433/2. Both embassies appeared, were heard and were accepted on the same day, or, at least, to be very cautious, in the same prytany. We would have thought that they went through the same drafting process. Nevertheless, the decrees, though more or less identical in content as far as they go, are different in wording and so are the oaths which they contain. The dissimilarities do not stop there. It is not merely that Leontini was given to an 'old-fashioned, rather slipshod mason' (Smart). The Rhegion treaty was put on a stele 0.092 m. thick and, we may calculate, 0.46 m. broad; the Leontini stele was 0.155 m. thick and 0.42 m. broad. The Rhegion treaty was carved in a chequer-pattern with vertical to horizontal in the proportion 3 to 2 with 33

letters to the line; the Leontini treaty was carved in a square chequer-pattern with seventeen letters to the line, and its stele is likely to have been a good deal the taller. The details of the dressing of the stones are quite different. We cannot think that the stelai were conceived as a pair. It is true that there is physical dissimilarity between the treaties with Chalkis and Eretria (*ATL* ii. D16 and 17 = *IG* i³ 39 and 40), even extending to the use of Ionic letters for the Eretria text, but in that case we do not find the drafting dissimilarities we have here. I see no temptation at all to assume that the || original texts are of the same date.

Meiggs and Lewis wrote of Rhegion 'There is very little difference in the letter-forms of the two hands. They could be, but need not be, contemporary' (pp. 171–2) and again 'they could be close contemporaries' (p. 174). Our intention, I think, was really to reinforce Accame's point that there appeared to be a time-gap between the original texts of Leontini and Rhegion, that is, we did not accept Meritt's view that both texts belonged *c.* 448. Smart paraphrases that 'one is not justified in placing any length of time between' the original and the reinscribed texts of Rhegion and proceeds on the assumption that they are in fact contemporary; Ruschenbusch quotes 'close contemporaries' and leaves out 'could be'. I do not think either of us has great confidence in his ability to distinguish between a text of 443 and one of 433, and I do not think any orthodox letter-form dater would see difficulty in putting the original Rhegion text *c.* 443; Smart's basic postulate thus disappears. My own view is that Rhegion may in fact belong around 444 or 443 and be connected with the foundation of Thurii. The best match I have so far found for its lettering is *IG* i² 359. Compare the photograph, *BCH* 91 (1967), 57, and note the 3 to 2 chequer-pattern. That text is dated before 443 by Meritt, *Athenian Financial Documents* 33; *IG* i³ 455 will date it 445/4 or 444/3.

I retain the view that earlier treaties with Leontini and Rhegion were reaffirmed with new prescripts in 433/2.

I do not accept the view (Bradeen and McGregor, *Studies in Fifth Century Attic Epigraphy* 121) that the stone-cutter intended us to read οἱ πρέσβες in Leontini line 1. I see no attempt to delete the punctuation after θεοί. I can only assume that there was an attempt, not completed, to transfer θεοί to a heading above line 1.

I note that Ruschenbusch has taken over two errors from our text of Leontini. Line 30 δες is a simple misprint for δεε; line 32 οτ is Hiller's reading, rightly changed by Meritt p. 89 to οπ. The errors have been corrected in the 1975 reprint of Meiggs–Lewis.

18

Entrenchment-clauses in Attic decrees

There is a personal anniversary for me besides that which we all celebrate, since it is just twenty-five years since Meritt answered a piece of undergraduate scepticism of mine, sent on to him by Tod, with infinite thoroughness and courtesy. In the years between, his care and patience with my troubles have never failed, my errors have been firmly dealt with, my occasional intransigences have been readily forgiven. I cannot begin to estimate my debt to him.

I define an entrenchment-clause as a clause inserted in a decree in an attempt to give it greater permanence and to limit any future attempt by those who might think it δεινὸν εἶναι εἰ μή τις ἐάσει τὸν δῆμον πράττειν ὃ ἂν βούληται[1] to reverse the decree by making it impossible or at least very difficult and dangerous to do so. There are a fair number of examples and the phenomenon seems to me of some importance for the development of Athenian ideas on legislation. In a period in which, at least for current legislation, the distinction between νόμος and ψήφισμα was far from clear, such clauses seem to constitute an interesting experimental approach to the problem of reconciling the demands of certainty and popular sovereignty. They are, however, as far as I can see, remarkably neglected. Since a clear paragraph by Busolt in 1920[2] I find nothing but a brief collection of largely non-Attic material by Tod,[3] and their non-appearance in Hignett's *History of the Athenian Constitution* and in Kahrstedt's discussion of the development of Athenian law-making[4] suggests that it may be justifiable to attempt to collect and understand the material.[5] As in all cases where parallel

* Published in Φόρος . . . *B. D. Meritt* (1974), 81–9 (J. J. Augustin Inc.). Entrenchment-clauses in Athenian and other decrees are discussed by Rhodes with Lewis, *The Decrees of the Greek States*.

[1] Xen. *Hell.* 1.7.12. [2] *Griechische Staatskunde* 463f. [3] *JHS* 54 (1934), 153–6.

[4] *Klio* 31 (1938), 1–32.

[5] I am indebted to A. Andrewes and P. J. Rhodes for their criticisms of a first draught.

material is brought together, the conjunction may produce the occasional interesting by-product. I confine the enquiry to Athens and to decrees. Sanctions against changes of law are of course at least as old as written law everywhere; I commend the Locrian τεθμός, ML 13, lines 7–16, as an early and drastic example.

Sanctions of this kind will have influenced the earliest Attic example, a decree of the deme Sypalettos, *IG* i², 189 = i³ 245, lines 5–12, which I would date 470–460 BC, ἄν τις ἐπι[φσ]|εφίσει λέχσεως [πέ|ρ]ι ἢ δόσεος ἀνα[. . | . . [2–3] .]ος πέρι, ὀφειλ[έτ|ο] χιλίας δραχμ[ὰς | τ]ῶι κωινῶι τῶι Σ[υπ|α]-λεττίον τον α[. | .]τειον.[6] It is not very clear what is going on, though *SEG* xxiii 22 = *IG* i³ 247, lines 2–6, [χρ]εμά[το]ν μὲ ἔ|[ναι . . . . [8] . . . .]ν μεδὲ δόσι|[ν . . . . . [10] . . . . .] ἐπὶ διαλέχ|[σει . . . [6] . . . ἐπ]ὶ τέχνει με|[δὲ μεχανεῖ με]δεμιᾶι, may conceal some sort of parallel, but it is clear enough that putting to the vote a proposal for changing existing arrangements will incur a fine. There is a similar provision in *SEG* xxiii 22 itself, a decree of a *genos* or a phratry, perhaps 460–440 BC, || where lines 10–14 read ἐὰν |[δέ τις ἐπιφσεφ]ίζει ἄλλ[α | παρὰ τὰ νῦν ἐφσ]εφισμένα |[. . . . . [12] . . . . . ε]ὐθυνέσθ|[ο with the continuation unrecoverable.[7]

Let us turn to public decrees. There is an initial distinction to be drawn between entrenchment-clauses which are designed merely to make action difficult and those which are designed to make it impossible. The first is represented textually for us only by the second decree of Kallias (*ATL*, D2; ML 58.B; *IG* i³ 52.B) of 434/3 BC.

(1) τοῖς δ]ὲ ἄλλοις χρέμα[σιν τοῖ]ς τῆς Ἀθεναίας το[ῖς τε νῦν ὄσι]-
[ν ἐμ πόλει κ]αὶ *h*άττ' ἂν τ[ὸ] λο[ιπὸν ἀν]αφέρεται μὲ χρῆσ[θ]α[ι μεδὲ ἀπα]-
[ναλίσκεν ἀ]π' αὐτῶν ἐ[ς] ἄλλο μ[εδὲν ἢ] ἐς ταῦτα *h*υπὲρ μυ[ρ]ί[ας δραχμὰ]-

[6] This is revised from that *IG* i² text, with changed line-division. The unintelligibility of what precedes has been increased by the loss of about five lines once inscribed on the corona. Wilamowitz suggested ἀλ(λ)ά[χ|σε]ος after a reading by Lolling; I prefer Kirchhoff's reading. I doubt Hiller's τῶν ἀ[σσ]|τείον.

[7] In a lease by the deme Aixone in 345 BC (*IG* ii² 2492, lines 29–31) the contract merely provides that anyone who proposes or puts to the vote a revision of the lease shall be liable to have a δίκη βλάβης brought against him by the lessees, that is, under state jurisdiction.

15 [ς ἢ ἐς ἐπισκ]ευὲν ἐάν τι δέε[ι· ἐς ἄλλ]ο δὲ μεδὲν χρẽσ[θ]α[ι τοῖς χρέμα]-
[σιν ἐὰμ μὲ τ]ὲν ἄδειαν φσεφ[ίσεται] ὁ δẽμος καθάπερ ἐ[ὰμ φσεφίσετ]-
[αι περὶ ἐσφ]ορᾶς· ἐὰν δέ τις [εἴπει ἢ] ἐπιφσεφί[σ]ει μὲ ἐ[φσεφισμένε]-
[ς πο τẽς ἀδεί]ας χρẽσθαι το[ῖς χρέμ]ασιν τοῖ[ς] τẽς Ἀθε[ναίας ἐνεχέ]-
[σθο τοῖς α]ὐτοῖς *h*οῖσπερ ἐά[ν τι ἐσ]φέρεν εἴπει ἢ ἐπιφ[σεφίσει.

Here there is no absolute bar on expenditure of Athena's money on purposes other than those specified; the procedure for doing so is merely more complicated and modelled on that laid down by an entrenchment-clause lost for us, that in the *eisphora*-procedure. We know nothing of the penalties for proposing *eisphora* or the use of Athena's money without *adeia*, nor for that matter very much about the method of voting *adeia* in either of these procedures. Hignett[8] inferred from other forms of *adeia* described in Dem. XXIV.46 that a quorum of 6,000 was necessary. This is probably right, but I suppose that even that passage, ἂν μὴ τῆς ἀδείας δοθείσης, καὶ ταύτης μὴ ἔλαττον ἢ ἑξακισχιλίων ψηφισαμένων, might be held to suggest that Demosthenes knew of other forms of *adeia* in which the quorum was different.[9]

Two years or so later Athena's money came in for further attention and protection.

(2a) Thuc. II.24 (431 BC): καὶ χίλια τάλαντα ἀπὸ τῶν ἐν τῇ ἀκροπόλει χρημάτων ἔδοξεν αὐτοῖς ἐξαίρετα ποιησαμένοις χωρὶς θέσθαι καὶ μὴ ἀναλοῦν, ἀλλ' ἀπὸ τῶν ἄλλων πολεμεῖν· ἢν δέ τις εἴπῃ ἢ ἐπιψηφίσῃ κινεῖν τὰ χρήματα ταῦτα ἐς ἄλλο τι, ἢν μὴ οἱ πολέμιοι νηίτῃ στρατῷ ἐπιπλέωσι τῇ πόλει καὶ δέῃ ἀμύνασθαι, θάνατον ζημίαν ἐπέθεντο. τριήρεις τε μετ' αὐτῶν ἑξαιρέτους ἑκατὸν ἐποιήσαντο κατὰ τὸν ἐνιαυτὸν ἕκαστον τὰς βελτίστας, καὶ τριηράρχους αὐταῖς, ὧν μὴ χρῆσθαι μηδεμιᾷ ἐς ἄλλο τι ἢ μετὰ τῶν χρημάτων περὶ τοῦ αὐτοῦ κινδύνου, ἢν δέῃ.

(b) Thuc. VIII.15.1 (412 BC): τά τε χίλια τάλαντα, ὧν διὰ παντὸς τοῦ πολέμου ἐγλίχοντο μὴ ἅψασθαι, εὐθὺς ἔλυσαν τὰς ἐπικειμένας ζημίας τῷ εἰπόντι ἢ ἐπιψηφίσαντι ὑπὸ τῆς παρούσης ἐκπλήξεως, καὶ ἐψηφίσαντο κινεῖν καὶ ναῦς πληροῦν οὐκ ὀλίγας.

(c) The Sicilian Expedition, 415 BC (ML 78 = *IG* i^3 93, frs. *d* and *g*):

[8] *History of the Athenian Constitution* 236; cf. F. Quass, *Nomos und Psephisma* 43.

[9] I leave out of account the possible reference to *adeia* for *eisphora* in *IG* i^2, 42, lines 21–4 [revised by Lewis as *IG* i^3 41, lines 36–9], since I do not see how these lines can be restored even with the slightly longer line I think probable and I strongly suspect that the two fragments cannot in fact be placed in the physical contact current texts require. But the reference to the κυρία ἐκκλησία may well be relevant.

ἐ]π' ἄλλο ἔργον με[δ' ἐπ' ἄλλεν σ]τρατιὰν
[----------τ]ρισχιλίον· ἐ[ὰν δέ τις εἴπει] ἢ ἐπιφσ-
[εφίσει----------]ς ἐχσαιρ[. . . . . [14] . . . . .]ον μ[.]
[----------]ογ!τ[-----]. ||

(We cannot do much with this last passage. An entrenchment-clause is certainly present, but we cannot tell whether it concerns the ἐξαίρετα of 431 or a new fund, and cannot recover the procedure or penalties.)

The bar here is not merely procedural, but it is not quite absolute either. It can be breached in a given contingency; it was in fact breached before that contingency actually arose in its most specific form. There may indeed have been some contingency-formula, as well as the procedural complication, attached to the *eisphora*-procedure; if so, it was less stringent than this, since *eisphora* was in fact voted in 428.[10] I have considered the possibility of arguing that, since the death-penalty is here provided for touching part of Athena's money, the penalty for failure to go through the *adeia*-procedure before touching the bulk of it must have been less severe, but the fact that there is this strict contingency-formula here makes the inference unsound.[11] The death-penalty could also have been contemplated in 434/3.

The death-penalty also appears in another entrenchment-clause of uncertain date, which stands in need of reconsideration.

(3) The Coinage Decree (*ATL*, ii. D14; ML 45; *IG* i³ 1453):[12]

15 ---------- ὃ δὲ ἂν περιγ]ίγνηται ἀργυρίο
[. . . . . [22] . . . . . ἀποδό]σθαι ἢ τοῖς στρατ-
[ηγοῖς ἢ . . . . . [19] . . . . . · ἐπε]ιδὰν δὲ ἀποδοθῆι,
[. . . . . [29] . . . . .]αι καὶ τῶι Ἡφαίσ-
[τωι . . . . . [11] . . . . . · καὶ ἐάν τις εἶπηι ἢ] ἐπιψηφίσηι περ-

[10] Thuc. III.19.1.

[11] Those who hold the second Kallias decree to be later than 431 BC of course have to find a reason why there is no mention of the ἐξαίρετα in it. Mattingly (*BCH* 92 (1968), 457f.) offers the possibility that the ἐξαίρετα were kept physically separate. One might suggest, though I do not think anyone has done so, that this particular sum of 1,000 talents was not Athena's.

[12] I give the text of the Aphytis fragment, *ATL* ii. 63f. (cf. E. Erxleben, *APF* 19 (1969), 112–17, 132), which has a *stoichedon* line of 42 letters, and underline those letters which occur on the non-*stoichedon* large fragment from Syme (*ATL*, ii. 65; Erxleben, *APF* 133). That Tod (*JHS* 69 (1949), 104f.) and Klaffenbach (*ap.* Erxleben) made alternative proposals for line 20 ceases to be relevant, as will be seen. [In *IG* i³ 1453 this extract is C. 14ff.]

20 [ἰ τούτων, ὅτι ἔστι ξενικῶι νομίσμα]τι χρῆσθαι ἢ δανε-
[ίζεν, ἀπογραφέσθω αὐτίκα μάλα πρὸς] τοὺς ἕνδεκα· οἱ δ-
[ὲ ἕνδεκα θανάτωι ζημιωσάντων· ἐὰν] δὲ ἀμφισβητῆι, ἐσ-
[αγαγόντων ἐς τὸ δικαστήριον.]

All editors, down to and including Meiggs and myself, have thought that the entrenchment-clause marked a new start and dealt with the provisions of the decree as a whole. However, reconsideration of the passage together with passage 1 will explain why I think this view should be discarded and why I have included lines 15–19, shorn of most of their restorations.[13]

I have always been puzzled by two points. What was the purpose of saying, in a quite general statement about the use of non-Attic silver, not only that it should not be used, but also that it should not be lent? Was there any distinction to be drawn between χρῆσθαι and ἀπαναλίσκεν in the second decree of Kallias?[14] If we put the two passages together, it seems to me to emerge that the two points are one and that both restorations are wrong. In the second decree of Kallias we should replace ἀπα|ναλίσκεν by δαν|είζεσθαι. The Athenians are not to use Athena's || money, not even with a promise to repay.[15] In the Coinage Decree, δανε[ίζεν] should be expanded to δανε[ίζεσθαι], and the reference is not to non-Attic silver in general, but to a particular fund, so obscurely defined for us in lines 15–19 of the Aphytis fragment. I offer no alternative restoration of line 20, affirming only that it must refer to that particular fund. Thuc. II.24 (passage 2a) strongly suggests [ἰ τούτων . . . . . [14] . . . . . ἐς ἄλλο] τι χρῆσθαι. Passage 3 protects a particular fund, as did passages 1 and 2, but offers no escape, either by procedure or by naming a given contingency, and the penalty for breach is death.

The restorations of the entrenchment-clause itself, except in line 20, have remained unaltered since D. M. Robinson published the Aphytis fragment

[13] R. S. Stroud will shortly publish a reconsideration of lines 18–19, based on a new reading from the stone. [Published *CSCA* 7 (1974), 279–83, 298.]

[14] Since the resumptive phrases in lines 15 and 18 simply have χρῆσθαι, it is clear that there can be no major distinction between it and the second verb, whatever it was. But the second verb can have contained a more explicit definition as well as a simple reiteration.

[15] In expense-accounts, though we are in fact always dealing with loans, the verb used of the Athenians is invariably ἀναλίσκειν, the verb used of Athena's treasurers is nearly always παραδιδόναι. The exception is ML 77 = *IG* i³ 370, line 66, where the treasurers do for once δανείζειν; I once suggested (*Hesperia* 28 (1959), 246 = this volume, 261) that this might be the sign of a payment of an inappropriate fund.

in 1935.[16] Nevertheless, the restoration in line 21 now seems to me untenable. There is no trace of any Athenian legal procedure which involves ἀπογραφή of persons to the Eleven nor of any ἀπογραφή involving summary execution. However, though Robinson's text cannot be right, his commentary shows that he saw the truth. If the Eleven are responsible and they execute unless the criminal ἀμφισβητῇ (in which case he goes to the jury), the procedure is almost certainly ἀπαγωγή. See Arist. *Ath. Pol.* 52.1 on the Eleven: καὶ τοὺς ἀπαγομένους κλέπτας καὶ τοὺς ἀνδραποδιστὰς καὶ τοὺς λωποδύτας, ἂν μὲν [ὁμολογῶ]σι, θανάτῳ ζημιώσοντας, ἂν δ' ἀμφισβητῶσιν, εἰσάξοντας εἰς τὸ δικαστήριον, κἂν μὲν ἀποφύγωσιν, ἀφήσοντας, εἰ δὲ μή, τότε θανατώσοντας.[17] If we restore the Aphytis fragment

χρῆσθαι ἢ δανε-
[ίζεσθαι, ἀπαγωγὴν αὐτοῦ εἶναι πρὸς] τοὺς ἕνδεκα,

we shall at any rate be right in sense and procedure.[18] There is a second possibility, a good deal less likely, but at least more likely than ἀπογραφή, and that is ἔνδειξις.[19] ἔνδειξιν has the same number of letters as ἀπαγωγήν, the Eleven dealt with some ἐνδείξεις, though we are not very sure which, and there are two passages about fifth-century procedure where ἀπαγωγή and ἔνδειξις seem almost inextricably linked, Arist. *Ath. Pol.* 29.4 (passage 8b below) and Plat. *Apol.* 32B7. But, except in the revolutionary situation of *Ath. Pol.* 29.4, there is seldom anything very automatic about what follows on ἔνδειξις, and, as far as I know, references to ἀμφισβήτησις by the accused in this sort of situation are confined to ἀπαγωγή. ἔνδειξις will have its turn in this article, but it has not yet come. It is ἀπαγωγή which is envisaged here, and it is clearly highly probable that this will also be the case in the decree about the ἐξαίρετα.

If we are dealing with ἀπαγωγή, entrenchment-clauses involving it amount to an assertion that anyone involved in the proposing or putting to the vote of the illegal motion belongs to the class of κακοῦργοι, along with

[16] *AJP* 56 (1935), 154.

[17] See also the various passages collected in Sandys' notes and apparatus and, in general, Lipsius, *Das attische Recht* 77–81, 317–31; Gernet, *Anthropologie de la Grèce antique* 315–25; Harrison, *The Law of Athens* i. 222–9.

[18] Mattingly (*per epist.*) suggests ἀπαγέσθω αὐτίκα μάλα. In theory, of course, all ἀπαγωγαί are instantaneous anyway, but see Harrison, *The Law of Athens* i. 224–5.

[19] See Harrison, *The Law of Athens* i. 229–31.

thieves, kidnappers, burglars and footpads, and that his guilt is so flagrant that it merits summary execution.[20] (The particular train of thought which brings these particular proposals into this class may lie in the view that to touch these funds was tantamount to ἱεροσυλία, a qualification for ἀπαωγή not recognised by Aristotle, the || lexicographers, Lipsius or Harrison, but reasonably affirmed by Gernet.[21]) His behaviour is so public that he is inevitably caught ἐπ' αὐτοφώρῳ, another characteristic of ἀπαγωγή-cases. It is interesting to find the doctrine revived almost in the same language in the 350s, even when the procedure against illegal proposals has totally changed, in Dem. XXIV.65: ὥσπερ τοίνυν, ὦ ἄνδρες Ἀθηναῖοι, τῶν περὶ τἄλλα κακούργων τοὺς ὁμολογοῦντας ἄνευ κρίσεως κολάζειν οἱ νόμοι κελεύουσιν, οὕτω δίκαιον καὶ τούτου, ἐπειδὴ τοὺς νόμους κακουργῶν εἴληπται, μὴ δόντας λόγον μηδ' ἐθελήσαντας ἀκοῦσαι καταψηφίσασθαι· ὡμολόγηκε γὰρ θατέρῳ νόμῳ τόνδε τιθεὶς ἀδικεῖν. Timokrates, by openly proposing a law contrary to a law he has previously passed, confesses his crime, has brought himself within the class of κακοῦργοι and deserves summary condemnation.[22]

I turn now to other cases in which the intention is not to protect a fund, but a more general decree or part of a decree.

(4) The Brea Decree, *c.* 445 BC (*IG* i² 45; ML 49; *IG* i³ 46.24ff.):

20 ἐ]ὰν δέ τις ἐπιφσεφίζει παρὰ τὲ[ν στέλ]-
[εν ἢ ῥρέ]τορ ἀγορεύει ἢ προσκαλῆσθα[ι ἐγχερ]-
[ῆι ἀφαι]ρῆσθαι ἢ λύεν τι τῶν *h*εφσεφι[σμένον],
[ἄτιμον] ἔναι αὐτὸν καὶ παῖδας τὸς ἐχς [ἐκένο]
[καὶ τὰ χ]ρέματα δεμόσια ἔναι καὶ τῆς [θεῶ τὸ ἐ]-

[20] Cf., e.g., besides *Ath. Pol.* 52.1, cited above, Dem. XXXV.47: τοιχωρύχους καὶ κλέπτας καὶ τοὺς ἄλλους κακούργους τοὺς ἐπὶ θανάτῳ οὗτοι εἰσάγουσιν; Lipsius, *Das Attische Recht* 77–9, 320; Harrison, *The Law of Athens* i. 223–4.

[21] *Anthropologe de la grèce antique* 322 n. 82. The relevant passages are Xen. *Mem.* 1.2.62, *Apol.* 25; Plat. *Rep.* 1.344B, VIII.552D, IX.575B. None of these passages mentions ἀπαγωγή, but the death-penalty asserted for ἱεροσυλία and the company it keeps leave little doubt. Rhodes draws my attention to the Hypothesis to Dem. XXV, para. 1, but, since it is the prytaneis who are involved and the case lands up in the assembly, it is clearly possible that ἀπάγουσι is untechnical there. A quite different law about ἱεροσυλία appears in Xen. *Hell.* 1.7.22.

[22] That this strand in Athenian thought can be traced so far means, I think, that there is nothing flatly impossible in the belief of the scholiast on Dem. 1.1 that Euboulos made proposals to amend the theoric law punishable by death, but I agree with Cawkwell, *JHS* 83 (1963), 59f., that the evidence is not good enough to prove it.

25 [πιδέκα]τον, ἐὰμ μέ τι αὐτοὶ *h*οι ἄποικ[οι . . . .]
[. . . . δέ]ονται.

It would have been more considerate if Meiggs and I had offered a translation of the first clause: 'If any (epistates) puts (anything) to the vote against the stele or a rhetor proposes or tries to issue a summons with a view to removing or repealing anything of what has been voted . . .' Unless the colonists raise points themselves, attempts to disturb the decree will incur ἀτιμία for the offender and his descendants and confiscation of property. The formula is unparalleled, though there are points of resemblance to passage 9 below.

(5) The Assessment of 425 BC (*ATL* A9; ML 69; *IG* i³ 71):

31 [κα]ὶ ἐάν τις ἄλλος δι[δῶι φσῆφον τῆ]ϙι [πόλεσι μ]ὲ ἔναι τ-
[ὰς] τάχσ[εις κατὰ Π]ϙ[ναθέναι]α τὰ μ[εγάλα] ἐπὶ τῆς πρυτανεί[ας
*h*έτις ἂν πρ]ότε [πρυτα]νεύει, ἄτ-
[ι]μος ἔσ[το καὶ] τὰ χ[ρέματα] αὐτῶ δ[εμόσι]α ἔσ[τ]ο καὶ τῆς θεῶ [τὸ
ἐπιδέκατ]ον.

Another unparalleled formula and not a very satisfactory one. The clause ought to refer to a proposer since the role of the πρυτάνεις has been amply covered in the previous five lines and we now have ἄλλος.[23] Meritt and West[24] saw this and translated their restoration, printed above, 'and if anyone else introduces a motion'. I confess that I have only just noticed that this is what the restoration is supposed to mean. I have always translated 'puts a motion to the || vote', which is its almost universal sense.[25] If we require Meritt and West's sense, and I think we do, we should at least amend the

[23] The older restoration, e.g. *IG* i², 63, line 31, covered both eventualities (κα]ὶ ἐάν τις ἄλλος δι[αχειροτονέσε]ι [ἓ εἴπει μ]ὲ ἔναι; consideration of earlier literature suggests that we are to take ἄλλος as ἄλλως), but this is almost certainly an impossible reading; see Meritt and West, *The Athenian Assessment of 425 B.C.* 11.

[24] *Ibid.* 48; cf. *ATL* iii. 72.

[25] Most epigraphic instances are gathered in *IG* ii² 4.i, p. 61. Lys. XV.2, 4, Dem. XXVIII.87, LVII.13, 15, LIX.90. I have not so far found it before the middle of the fourth century (Lysias XV is of course problematic). LSJ compares Dem. XXI.188 for this usage, wrongly. But though the jury is the subject there, the phrase does not of course simply mean 'vote' in this passage, but 'grant your vote to his children' (so Goodwin *Demosthenes Against Midias*). The only other instance which diverges slightly from the norm is Dem. LIX.109, where the subject is not an official and the sense 'no one gave an opportunity to vote about her' is an easy extension of the normal usage.

restoration to δι[δῶι γνόμεν], which gives the right meaning. But it is desperately rare,[26] and I think it quite likely that further recasting of the line may be necessary. Again, attempts to disturb this clause of the decree will bring ἀτιμία, not this time explicitly on the descendants, and confiscation.

(6) Aphytis, 430–420 BC (*IG* i² 58; i³ 63; not *stoichedon*):

[ἐὰν δέ τις]
[ἒ] ἄρχ[ον ἒ ἰδιότες εἴπει ἒ ἐπιφσεφίσει παρὰ τ]-
όδε τὸ φ[σέφισμ]α[27] [*h*ος δεῖ Ἀφυταίος ἀφαιρεῖσ]-
θαι ταῦτα, ὀφελέτο [μυρίας δραχμὰς ἱερὰς τεῖ Ἀθ]-
εναίαι.

At any rate the outlines are clear. This time there is no question of ἀτιμία or confiscation. A specific sum is necessary for the fine, and a fine of 10,000 drachmae is mentioned elsewhere in the text (line 19).

(7) The treaty with Perdikkas, date controversial (*IG* i² 71; *SEG* x 86; *Staatsverträge*, 186; *IG* i³ 89):

52 [. . . . . [22] . . . . .]ονται ἀμφότ[εροι . . . . . [17]
. . . . . ἐ]ναντίον τõι[. . . . . [34]
. . . . .]
[. . . . . [22] . . . . .]ας σφõν αὐτ[õν . . . . . [18]
. . . . .] ἒ τὸν ἐπιφσεφί[σαντα . . . . . [29]
. . . . .]
[. . . . . [21] . . . . .]ερίον *h*ένεκ[α . . . . . [14]
. . . . . καὶ τ]ὰ χρέματα αὐτõ δε[μόσια κτλ.][28]

Potentially very interesting, but nearly intractable. Here the entrenchment-clause is used to guarantee the interests of someone wholly external to Athens. It is of course not clear where it starts. It is possible that]ονται ἀμφότ[εροι covers the possibility of changes agreed by both parties, Athens and Perdikkas (Arrhabaios has not appeared in the text at this point), just as the last clause of passage 4 covered the possibility that the Brea colonists might want changes. ἐ]ναντίον τõι [might be τõι[δε τõι φσεφίσματι, as

[26] Dem. XXIV.13 is a clear case, but I have found no other.

[27] I have added the *alpha*.

[28] I have had much help from Edson's unpublished text, and the line-numbering is his. My own view is that the most likely line-length is 97 letters, rather than the 100 letters

an alternative to παρὰ τὲν στέλεν or παρὰ τόδε τὸ φσέφισμα.]ας σφῶν αὐτ[ῶν is not readily intelligible. τὸν εἰπόντα] looks attractive before ἢ τὸν ἐπιφσεφί[σαντα, which I prefer to the τὸν ἐπιφσεφί[ζοντα of Davis.[29] In line 54 *h*ένεκα was taken as postpositive even before Davis put the fragments together, and he naturally followed suit. I think this difficult.]εριον would have to be the genitive plural of some word indicating criminal activity. The reverse-indices do not suggest anything very obvious. μοχθ]ερίον and πον]ερίον are not sufficiently technical and the plural would be odd. Even if something could || be found, our troubles would not be over. Confiscation inevitably presupposes ἀτιμία, for which room must be found. Davis restored [ἄτιμον ἔναι αὐτὸν], which is one letter too long. We ought to be following an infinitive construction through from the accusative participles, which makes [ἄτιμος εἶε αὐτὸς] (Edson) or [ἄτιμος ὑπαρχέτο], without the aspirate, distinctly improbable. In this situation, I think we are forced to make *h*ένεκα prepositive and]εριον accusative. [δικαστ]έριον then becomes the most likely restoration. The gap in the middle of the line could be, e.g., [τῶ κακοργέματος]. Nothing very certain emerges: ἀτιμία and confiscation, possibly reference to a court.

I close the fifth-century examples with what may be described as an entrenchment-clause against entrenchment-clauses, that proposed by the συγγραφεῖς of 411.

(8a) Thuc. VIII.67.2: καὶ ἐσήνεγκαν οἱ ξυγγραφῆς ἄλλο μὲν οὐδέν, αὐτὸ δὲ τοῦτο, ἐξεῖναι μὲν Ἀθηναίων ἀνατεὶ εἰπεῖν (Sauppe; ἀνατρέπειν ACEF; ἀνειπεῖν cett.)[30] γνώμην ἣν ἄν τις βούληται· ἣν δέ τις τὸν εἰπόντα ἢ γράψηται παρανόμων ἢ ἄλλῳ τῳ τρόπῳ βλάψῃ, μεγάλας ζημίας ἐπέθεσαν.[31]

(b) Arist. *Ath. Pol.* 29.4: οἱ δ' αἱρεθέντες πρῶτον μὲν ἔγραψαν ἐπάναγκες εἶναι τοὺς πρυτάνεις ἅπαντα τὰ λεγόμενα περὶ τῆς σωτηρίας ἐπιψηφίζειν, ἔπειτα τὰς τῶν παρανόμων γραφὰς καὶ τὰς εἰσαγγελίας καὶ

suggested by Edson, who is followed by *ATL* iii. 313 n. 61, since the normal phrase is τὸν γραμματέα τῆς βολῆς and the second τὸν interpolated by *ATL* in their line 51 is superfluous.

[29] *AJA* 30 (1926), 185.

[30] ἐξεῖναι μὲν ἀζήμιον εἰπεῖν, Wilamowitz, *Hermes* 12 (1877), 336 n. 7, should perhaps not be forgotten.

[31] Cf. θάνατον ζημίαν ἐπέθεντο in passage 2a *supra*.

τὰς προσκλήσεις (προκλήσεις pap.) ἀνεῖλον, ὅπως ἂν οἱ ἐθέλοντες Ἀθηναίων συμβουλεύωσι περὶ τῶν προκειμένων· ἐὰν δέ τις τούτων χάριν ἢ ζημιοῖ ἢ προσκαλῆται ἢ εἰσάγῃ εἰς δικαστήριον, ἔνδειξιν αὐτοῦ εἶναι καὶ ἀπαγωγὴν πρὸς τοὺς στρατηγούς, τοὺς δὲ στρατηγοὺς παραδοῦναι τοῖς ἕνδεκα θανάτῳ ζημιῶσαι.

I do not bother to argue that (a) paraphrases (b). We have already noted the close connection of ἔνδειξις and ἀπαγωγή in (b). The generals are not normal intermediaries, but perhaps appropriate to the strained times. ἔνδειξις in the sense of literal pointing-out to them will save an accuser from performing his own ἀπαγωγή.

In (b) three categories of legal procedure are made invalid, and we perhaps ought to look at them to see if they can be made to fit those entrenchment-clauses which we have not classified as treatable by ἀπαγωγή; we have after all not yet defined the procedure followed in passages 1, 4–7. One certainly might find points of disagreement with Sandys' comments on the passage, particularly on εἰσαγγελία, where he followed a slightly peculiar doctrine of Lipsius which took no account of this passage at all. But I think we can save ourselves the trouble quite simply by affirming that none of the three categories provides the automatic procedure and fixed penalties which we require for these entrenchment-clauses.

Lipsius' view[32] appears to have been that the appropriate procedure was ἔνδειξις.[33] This normally applied to those who, despite ἀτιμία, usually but not invariably arising from debt to the state, attempted to continue their activities as officials or citizens. The train of thought ought to be that, as soon as the illegal act is performed, the offender is a public debtor. I am not altogether happy about this. Lipsius asserts the general proposition that ἀτιμία and ἔνδειξις go together in laws and decrees, but this seems to be a generalisation from one case, the Law of Leptines,[34] where it is not even quite certain that ἄτιμος ἔστω καὶ ἡ οὐσία δημοσία ἔστω . . . . εἶναι δὲ καὶ ἐνδείξεις καὶ ἀπαγωγάς· ἐὰν δ' ἀλῷ, ἔνοχος ἔστω τῷ νόμῳ ὃς κεῖται, ἐάν τις ὀφείλων ἄρχῃ τῷ δημοσίῳ should be taken as a continuous quotation. A more helpful instance is the law in Dem. XXIV.22, which, like Lipsius, I accept as genuine: ἐὰν δ' οἱ πρυτάνεις μὴ ποιῶσι κατὰ τὰ || γεγραμμένα τὴν ἐκκλησίαν ἢ οἱ πρόεδροι μὴ χρηματίσωσι κατὰ τὰ γεγραμμένα, ὀφείλειν

[32] *Das attische Recht* 332. [33] See also Harrison, *The Law of Athens* i. 229–31.
[34] Dem. XX.156.

τῶν μὲν πρυτάνεων ἕκαστον χιλίας δραχμὰς ἱερὰς τῇ Ἀθηνᾷ, τῶν δὲ προέδρων ἕκαστος ὀφειλέτω τετταράκοντα δραχμὰς ἱερὰς τῇ Ἀθηνᾷ. καὶ ἔνδειξις αὐτῶν ἔστω πρὸς τοὺς θεσμοθέτας, καθάπερ ἐάν τις ἄρχῃ ὀφείλων τῷ δημοσίῳ. This at least shows the possibility that the penalties envisaged in our fifth-century entrenchment-clauses were enforceable by ἔνδειξις, and I think this very likely. But in only one of our public clauses (no. 6) is the penalty a simple fine, and there is no mention of consequent ἀτιμία.[35] My inclination is to say that no procedure is envisaged in the entrenchment-clauses for creating ἀτιμία as opposed to enforcing it. The offender makes himself ἄτιμος and liable to confiscation[36] by his simple act.

Did the συγγραφεῖς of 411 overlook procedures of this kind? I think not. ἐὰν δέ τις τούτων χάριν ἢ ζημιοῖ . . . will cover ἀπαγωγή and ἔνδειξις processes as well as the results of γραφαὶ παρανόμων and εἰσαγγελίαι, although Sandys confines ζημιοῖ to the latter.

The procedures envisaged by fifth-century entrenchment-clauses, then, are purely automatic. There is no attempt to bring them under the scope of any law, though existing legal procedures may be used to enforce them. In fact, it is hard to think that their proposers were really worried about any distinction between law and decree at all. If there was any proposal or clause in a proposal which seemed to merit tighter safeguards, they inserted them, doubtless with ample precedent in real laws. The θεσμοί of Drakon after all contained such a clause: ὃς ἂν ἄρχων ἢ ἰδιώτης αἴτιος ᾖ τὸν θεσμὸν συγχυθῆναι τόνδε ἢ μεταποιήσῃ αὐτόν, ἄτιμον εἶναι καὶ παῖδας [ἀτίμους] καὶ τὰ ἐκείνου.[37] Drakon doubtless meant something much worse by ἀτιμία, but the tradition went on.

It went on in the fourth century too, despite the more rigid distinction between law and decree. But our last example may well be thought to have fifth-century practice rather closely present to its mind.

(9) The manifesto of the Second Athenian Confederacy, 377 BC (*IG* ii² 43; Tod 123):

[35] Rhodes rightly wonders whether ἔνδειξις was ever a normal method of enforcing the payment of a fine before the ἀτιμία had arisen, but there is clearly some assimilation of the procedures.

[36] The confiscation has to be enforced by ἀπογραφή of his property; see *Ancient Society and Institutions . . . Victor Ehrenberg* 182ff.

[37] Dem. XXIII.62. Considering the passages before us, one may well think that simply removing ἀτίμους with Taylor is not enough to cure the corruption.

ἐὰν δέ τ-
ις εἴπηι ἢ ἐπιψηφίσηι, ἢ ἄρχων ἢ ἰδιώτη-
ς, παρὰ τόδε τὸ ψήφισμα ὡς λύειν τι δεῖ τ-
ῶν ἐν τῶιδε τῶι ψηφίσματι εἰρημέν[ων, ὑ]-
παρχέτω μ[ὲν] αὐτῶι ἀτίμωι εἶναι, καὶ [τὰ]
[χρ]ήμα[τα αὐ]τοῦ δημόσια ἔστω καὶ τῆς θ[εοῦ]
τ[ὸ] ἐπιδ[έκα]τον, καὶ κρινέσθω ἐν Ἀθην[αί]-
[ο]ις καὶ τ[οῖς] συμμάχοις ὡς διαλύων τὴν
συμμαχία[ν, ζ]ημιούντων δὲ αὐτὸν θανάτω-
ι ἢ φυγῆι οὗ[περ] Ἀθηναῖοι καὶ οἱ σύμμαχο-
ι κρατοῦσι[ν· ἐὰν] δὲ θανάτου τιμηθῆι, μὴ τα-
φήτω ἐν τῆ[ι Ἀττι]κῆι [μ]ηδὲ ἐν τῆι τῶν συμ-
μάχων.

In the first half there are obvious links with passages 4 and 6, probably inevitable in a clause designed to safeguard a whole decree rather than part of it. No distinction, I think, should be sought between ὑπαρχέτω μὲν αὐτῶι ἀτίμωι εἶναι and a simple ἄτιμος ἔστω or we shall be left || with non-automatic ἀτιμία and automatic confiscation, which seems improbable. What is new is the procedure envisaged in the second half, but that has clear affinities with traditional procedures for προδοσία,[38] which is not really my subject.[39]

In looking at these formulae, I have from time to time thought I saw possible new dating arguments for controversial texts, but none of them will really stand. I do think that the failure of the Brea Decree (passage 4) to use ἐάν τις εἴπῃ ἢ ἐπιψηφίσῃ is an argument for a relatively early date. That the Assessment Decree of 425 also lacks it is of no consequence, since the aim

[38] Cf., e.g., Xen. *Hell.* 1.7.22.

[39] I agree with Larsen, *Representative Government in Greek and Roman History* 63–4, that the view of Marshall, *Second Athenian Confederacy* 35–6, that this passage envisages a joint Athenian and allied court is probably wrong. My own inclination is to see here a simple extension of normal Athenian εἰσαγγελία-procedure at a time when the assembly normally had the final voice in εἰσαγγελία (Lipsius, *Das attische Recht* 188ff.). Just as the allied συνέδριον acted as a third chamber in other imperial matters, it might well be that it was envisaged that it would participate in these imperial εἰσαγγελίαι. Ἀθηναῖοι καὶ οἱ σύμμαχοι will mean exactly what it does, e.g., in *IG* ii² 97 = Tod 127 lines 34–5, all three chambers. This will not fit very well with Rhodes' attempt to diminish the part played by the assembly in fourth-century εἰσαγγελίαι (*The Athenian Boule* 164–71), but I am as yet far from convinced by him about this.

there is to make sure that something is done, not that it is not done. Its presence in the Coinage Decree could not however be a strong argument for a late date, since an early use of what became the standard formula is perfectly possible. The more serious argument against those of us who have held to an early dating for the Coinage Decree is the presence of the death-penalty for breach, not otherwise attested until 431. As far as we can tell, there is no escape procedure and no contingency under which alternative expenditure of this mysterious fund will be possible. Without knowing more about the fund, we can hardly say more than that the Coinage Decree is firmly, even harshly, formulated, but not all of us believe that firmness and harshness were new phenomena in Athens in the period of Kleon's ascendancy.

19

Apollo Delios

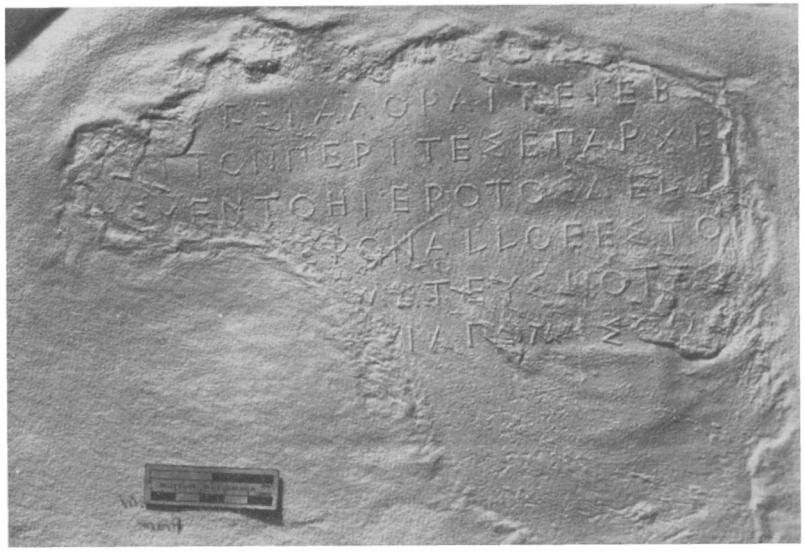
The fragment which I publish here by the kind permission of Dr J. Papademetriou lies in the courtyard of the Piraeus Museum. Of its origins nothing can be said. It does not appear in the Museum inventory, and all that can be affirmed for certain is that it was already in the Museum in January 1937, when Mr J. G. Griffith made a squeeze which is now in the Ashmolean Museum. This squeeze besides its chronological value is also helpful, in that it shows rather more than can now be read on the stone.[1]

Fragment of Pentelic marble with left-hand edge and original rough-picked back preserved. Height: 0.352 m; width: 0.349 m; thickness: 0.152 m. Uninscribed left margin of 0.026 m. Uninscribed space below last line of at least 0.110 m. Cut with two chisels of 10 mm and 6 mm, the smaller being employed for the cross-bars of *epsilon*, *alpha*, and the aspirate, the horizontal and right vertical of *pi*, and all three strokes of *upsilon* and *sigma*. Horizontal chequer: 0.0137 m; vertical chequer: 0.0194 m. See Plate 1 (photograph of squeeze).

. . . νο̣ . . 5 . . ΣT̄Ο̣Ε̣
. . ι τε̃ι ἀγορᾶι τε̃ι εβ_[------- περ]
ι̣ττὸν περὶ τε̃ς ἐπάρχε[ς------ ἐπισκ]
ευὲν τõ Ηιερõ τὸς ΔΕΛ̣[---------]
[. . . Ηιε]ρὸν ἄλλο ἔ ἐς το[---------]
[. Ηο γραμ]ματεὺς Ηο τε̃ς [βολε̃ς ----- Ηοι]
[δὲ πολετα]ὶ ἀπομισθοσ[άντον
vacat

* Published in *BSA* 55 (1960), 190–4 with pl. 51*a*. Lewis's reconstruction of *IG* i² 128 and this fragment is repeated at *IG* i³ 130; the squeeze is now in the Centre for the Study of Ancient Documents, Oxford.

[1] I am grateful to Professor H. T. Wade-Gery for reading a first draught.



1 Apollo Delios: *IG* i³ 130, frag. b (squeeze at Oxford).

Line 1 is read entirely from the stone, the squeeze not being adequate at this point.

Line 2. The last visible cutting is part of a horizontal on the bottom line. Of the letter before I can now only see the vertical and the start of a stroke from the bottom of the vertical. The squeeze shows a complete and certain *beta*.

Line 4. The last *iota* is uncertain, but any other letter would show some trace.

Line 6. The *alpha* and the last two letters are only on the squeeze.

Line 7. The *theta* is only on the squeeze.

That we have the end of an Attic decree from the last forty years of the fifth century needs no demonstration. The neat hand seems to me to belong around 430. *IG* i² 97 = i³ 60, now dated by Meritt[2] in the opening years of the Archidamian War, appears to show the same hand working with less space at his disposal; the prescript of the Leontini treaty of 433/2 (*IG* i² 52 = i³ 54)

[2] *Studies presented to D. M. Robinson* ii. 298ff. Treu's objections (*Historia* 3 (1954–5), 58) are based on dogma, not on the text. Neither considers the lettering.

has him with plenty of space to spare. *IG* i² 53 = i³67, of which the date and purport have been much disputed,[3] is also very close.

We have some kind of public regulation of a cult, its *hieron* and its *eparche*. Line 2 is a puzzle. I can think of no solution, if we take the phrase following τῆι ἀγορᾶι together with it. The only solution which suggests itself to me is to take it as τῆι ἐβδ̣[ομῆι] with an omitted aspirate. This || ought to be a date. The seventh day implies Apollo.[4] Apollo in this neighbourhood will be Apollo Delios at Phaleron,[5] and that we may be on the right track is suggested by line 4. For if we start a new sentence after Ἱερῶ, and divide τὸς δὲ λι[, we shall have to restore something like λι[μενοφύλακας], and these are unknown in Attica.[6] τὸς Δελί[ος] is rather more hopeful.[7] Finally, lines 6–7 will suggest a line-length of about sixty letters if Ἱοι δὲ κολακρέται δόντον τὸ ἀργύριον follows the *poletai*-clause in the normal way, of about ninety letters if it precedes it as in *IG* i² 73 = i³ 153. 21–3. But a ninety-letter line would give the stele a width of well over a metre, which is unlikely in the extreme, and the shorter line is much more plausible.

On this fragment alone, one could hardly go farther. But it is worth noting that fifth-century decrees from Piraeus or Phaleron are not common, and that *IG* i² 128 = i³ 130. *a*, which is now lost, corresponds with our fragment in several respects. It has a line of fifty-two letters plus the space required for the name of the secretary, and, if the word θεοί was laid out symmetrically in line 1, it would have sixty. Its aspiration is inconsistent. It deals with the regulation of the cult of a male god and of its finances and with the building of his *hieron*. It was found between Zea and Mounichia, but this is not far for a stone from Phaleron to move in a sea-port. That its conventional date is *ante a. 428/7* has no probative value, for, as Hiller pointed out, nothing can be built on the name Lysikles. Nevertheless, the resemblances between it and our new fragment are striking enough to make it extremely tempting to take them as parts of the same document.

The last discussion of *IG* i² 128 is by Schlaifer in *HSCP* 51 (1940). His *exempli gratia* restorations are unfortunately not given in *SEG* x, and I reprint them here for ease of reference:

[3] See *SEG* x 46, xiii 9, and my note, *BSA* 49 (1954), 25 n. 27. [Some more recent studies are cited in *IG* i³.]

[4] Nilsson, *Primitive Time Reckoning* 333, 366.

[5] *IG* i² 310. 218–19 = i³ 383.153–4.

[6] The λιμενόφρουρος of *IG* ii² 133. 23 has been disposed of (Wilhelm, *Anz. Wien* 84 (1947), 196).

[7] One should, however, bear in mind Meisterhans's warning about the article with plural ethnics (*Grammatik der attischen Inschriften*³ 120 n. 12).

ΣΤ. 57

ἔδοχσεν τεῖ βολεῖ καὶ τõι δέμοι. Ἀντ[ιοχὶς ἐπρυτάνευε, . . [5] . . ἐγραμμάτ]
ευε, Στράτον ἐπεστάτει, Λυσικλεῖς εἶπ[ε· . . . . . [25] . . . . . ἐ]
πειδὲ χσυμβάλλονται οἱ ναύκλεροι [καὶ ἔμποροι οἱ ἐμ Περαιεῖ μίαν δρα]
χμὲν ἕκαστος ἀπὸ τõ πλοίο ὅπος ἂν τõ [Διὸς τõ Σοτῆρος τὸ ἐμ Περαιεῖ
ἱερὸ]
ν σõν ἔι τõι θεõι [κτλ].

This seems to me hardly satisfactory. Besides minor stylistic faults, it leaves τõ πλοίο quite unexplained and subordinates ὅπος ἄν, so common in Attic preambles as a justification, in an extremely unusual way. I offer here a combined text of both fragments, with due reserve.

θ ε [ο ί]

ἔδοχσεν τεῖ βολεῖ καὶ τ[õι] δέμοι, Ἀντ[ιοχὶς ἐπρυτάνευε, ... [8] . . .
ἐγραμμάτ]
ευε, Στράτον ἐπεστάτει, Λυσ[ι]κλεῖς εἶπ[ε· τõι Ἀπόλλονι τõι Δελίοι
θῦσαι, καὶ ἐ]
πειδὲ χσυμβάλλονται οἱ ναύκλεροι [Ϝοι παρὰ τὸ Φάλερον
Ϝορμιζόμενοι δρα]
χμὲν ἕκαστος ἀπὸ τõ πλοίο, ὅπος ἂν τὸ [ἀργύριον τὸ ἀπὸ τõν
δραχμõν γενόμενο]
ν σõν ἔι τõι θεõι, ἀποδιδόσθον οἱ π[ολεταὶ τὸ τẽς δραχμẽς τέλος ἐπὶ
τẽς πεμπ]
[τẽ]ς πρυτανείας ὅταμπερ τὸ τõν ἀπα[ρχõν τέλος μισθõσι· δõναι
δ' ἐς τὲν οἰκοδ]
[ομ]ίαν δεμοσίαι μὲν μεχρὶ πεντακο[σίον δραχμõν, ἰδίαι δὲ κατὰ
δύναμιν· τὸ δ]
[ἐ ἱερ]ὸν ποιẽσαι ὅς κάλλιστον, προσ[καλέσθο δὲ πρὸς τὲν βολὲν καὶ
ἐς τὸν δẽμ]
[ον ὁ νῦ]ν ἀρχιτέκτον καὶ ἐπισκευαζ[έτο ||
[. . . [6] . .] ταύτ[ην Ϝό]πος [ἄ]ν γίγνεται ἐ[
[. . . . . [17] . . . . .]μον οἳ ἂν ὅσι τ[
[. . . . . [16] . . . . . γ]ενόμενον καὶ [
[. . . . . [18] . . . . .]εον Ϝόπος ἂν [
[. . . . . [18] . . . . .]ρνἐ[
lacuna

Fragment *b*

. . . νο̣ . . [5] . . Σ̄Τ̄Ο̣Ε̣ [. . . . . [24] . . . . . καὶ τõι Ἀπόλλονι θύεν Φαλε]

[ρõ]ι τẽι ἀγορᾶι τẽι ἐββ̣[ομẽι τõ μενὸς . . . . [11] . . . . ἐὰν δέ τίς τι πράττει περ]

ιττὸν περὶ τẽς ἐπαρχẽ[ς, μνᾶν ἀποτινέτο· χσυμβάλλεσθαι δὲ καὶ ἐς τὲν ἐπισκ]

ευὲν τõ Ηιερõ τὸς Δελί[ος ἐὰν θέλοσι· καὶ μὲ ἔναι χρẽσθαι τõιδε τõι ἀργυρίο]

5 [ι ἐς Ηιε]ρὸν ἄλλο ἒ ἐς τὸ [Ἀπόλλονος τὸ Φαλεροῖ· τὸ δὲ φσέφισμα τόδε ἀναγράφσ]

[ας ὁ γραμ]ματεὺς Ηο τẽς [βολẽς ἐν στέλει λιθινẽι καταθέτο ἐν τõι Ηιερõι· Ηοι]

[δὲ πολετα]ὶ ἀπομισθοσ[άντον· Ηοι δὲ κολακρέται δόντον τὸ ἀργύριον. *vacat*]

vacat

Restorations and commentary

Line 3. Straton as *epistates* of the *prytaneis* must be from Antiochis (*PA* 12971). The only others of this name which I know from this tribe are *PA* 12977, 12982, and, much later, 12995.

My restoration for the opening of the decree is hardly certain, but such decrees must have the god in the first clause; cf. *IG* i² 76, 78, 94, 127 = i³ 78, 137, 84, 133. Once he is there, this is the most likely thing to be done for him. One would expect a reason to be given for the sacrifice as in i² 78, but there may be a motive for its absence, as we shall see.

Line 4. It is hardly necessary to refute once again the supposition that the *naukleroi* are a *collegium*. See, besides the references in the *editio minor*, Francotte, *L'Industrie dans la Grèce ancienne*, ii. 207.

My restoration for the rest of the line is modelled on that worked out by Wilhelm for the Sounion decree (*SEG* x 10 Addenda = *IG* i³ 8, lines 16–17) and that decree makes it clear what is wanted here. For Schlaifer's restoration does not make it clear how often a ship has to pay the drachma, and the procedures operating at Sounion and Phaleron are clearly parallel.

Line 5. Schlaifer, misled by the *IG* i² restoration [ἱερόν], says 'The lacuna certainly contained the name of the deity whose hieron was in question' and,

not surprisingly, has trouble with his word-order. I prefer to model on *IG* ii² 1172. 14, 1174. 1–2. I have taken τὸν δραχμὸν from Wade-Gery, preferring it to my τῆς ἐπαρχῆς, borrowed from fragment *b*, line 3. I doubt the possibility of distinguishing between ἐπαρχή and ἀπαρχή, but it is probably better not to bring them into too close conjunction.

Lines 6–7. *IG* i² restores τέλος doubtfully, but surely correctly. Cf., for example, Dem. XXIV.144, Aeschines 1.119, *Hesperia* 5 (1936), no. 10, p. 401, lines 134, 142.

[πεμπτῆ]ς is, of course, only restored because it fits my length of line, which [ἑβδόμε]ς and [δεκάτε]ς would also do, but I do not see the ground of the *IG* i² restoration [πρότε]ς. For even the *Athenaion Politeia*, our fullest account of the *poletai*, does not commit them to selling concessions in any prytany in particular. *IG* i² 94 = i³ 84 (ninth prytany) is good enough evidence to suggest that concessions might be sold at any time, and the fourth-century *poletai*-records make it clear that they were in fact so sold. I select [πεμπτῆ]ς because it gives the concessionaires time to organise themselves at the end of a sailing-season well before the next begins, but there could be no objection to [ἑβδόμε]ς. ||

Lines 7–8. The only real alternative to [οἰκοδομ]ίαν seems to me to be [θυσ]ίαν, and *IG* i² 78 = i³ 137. 6ff., where the *epistatai* spend μεχ[ρὶ μνᾶς: not restored in *IG* i³] on a sacrifice and citizens not less than a drachma, i² 79 = i³ 138 with its elaborate contribution-scale, and *SEG* x 64a = *IG* i³ 136. 35 (not restored in *IG* i³) are attractive parallels. But, if we follow them, the sacrifice is too soon over with no arrangements about the distribution of the meat or of the skins, and the building of the *hieron* begins too abruptly. 500 drachmai is admittedly not much for a state-contribution to a *hieron*, but we do not know how big the shrine was to be.

[κατὰ δύναμιν]. No epigraphical parallels that I know in this sort of context, but cf. i² 6 = i³ 6 *A*. 29–30 κατὰ τὲν δύνα[μιν], and the sense is clearly what is required.

Line 12. [οἱ δέμαρχοι τὸν δέ]μον οἳ ἂν ὅσι τ[ότε?]

Fragment *b*, line 3. [περ]ιττόν. Nowhere else in Attic inscriptions that I know, except, I suspect, in i² 107 = i³ 108. 8, where we should restore something like [μὲ πρ]άττειν περιτ[τόν] (not restored in *IG* i³).

Lines 4–5. The general sense is clear if we bear in mind the second Decree of Kallias (*ATL*, ii. D2 = *IG* i³ 52 *B*. 12ff.). I must confess that I would prefer ἐς τῦ[το ἀνεὺ τῆς βολῆς καὶ τῦ δέμο] (cf. i² 76 = i³ 78. 56), but see no way to fit it in.

It would be of importance to group all the fifth-century Athenian decrees where a private cult is taken over and regulated by the state, but while we still lack Raubitschek's commentary to his reconstruction of the Anakes-decree (*IG* i² 127 = *SEG* x 59 = *IG* i³ 133) and the detailed justification of his redating to around 412 of the Bendis-decree, it is hardly possible. Meanwhile, we have Schlaifer,[8] who seems to me to be excellent on Theseus and Asklepios, and Ferguson[9] and Nilsson[10] on Bendis. Some thought should also be devoted to the Sounion-decree (*SEG* x 10 = *IG* i³ 8), although it will not take us far, and above all to the Eleusis-decree (*IG* i² 6 = i³ 6), though this again will be handicapped until the related fourth-century decree[11] is available for study.

To confine ourselves to the decree before us, it would appear that the state for some reason decides to take an interest in the cult of a god, whom I have suggested to be Apollo Delios. There is already an informal cult, and shipowners contribute a voluntary levy to it. The collection of this levy is now put under the public supervision of the *poletai*. Though we have no information from the decree as to who is put in charge of the fund thus accumulated, the control of the property of Apollo Delios rests in the hands of the Treasurers of the Other Gods in 429/8[12] and they probably make loans from it in 423/2. There are two unidentified Apollos in the *logistai*-inscription at *SEG* x 227 = *IG* i³ 369. 68–9, 71–2. Lines 68–9 could be restored Ἀπόλλον[ος Δελίο ΡΗΗ|ΓΙΙΙ], τούτο τόκος Γ Η Η Η, but really all that is certain is that one of these two should be Apollo Delios.[13]

A shrine is to be built, largely, presumably, from voluntary subscriptions, but the state will make up the amount by up to 500 drachmai, and will provide a state-architect to conduct the work. The lacuna may have contained the provisions for regular sacrifices, which seem to be in question at the beginning of fragment *b*. The decree ends with penalties for mishandling the *eparche*, a provision that the people of Delos shall be allowed (and probably

[8] *HSCP* 51 (1940), 233ff. [9] *Hesperia* Supp. viii (1949), 131ff., particularly 142, 157ff.

[10] *Cults, Myths, Oracles and Politics* 45ff.

[11] Schweigert, *AJA* 51 (1946), 287ff. [See now K. Clinton, *Hesperia* 49 (1980), 258–88 = *SEG* xxx 61.]

[12] *IG* i² 310. 218–19 = i³ 383. 153–4.

[13] Compare also *IG* i² 94 = i³ 84, where the *apodektai* are to pay receipts belonging to Kodros, Neleus, and Basile to the Treasurers of the Other Gods κατὰ τὸν νόμον. What law? Not, I think, the law about *temene* (*ibid.* line 25), because this might involve Athena's property too. Rather, I suspect, the first Kallias Decree (*ATL*, ii. D1 = *IG* i³ 52 *A*. 24–7).

encouraged) to subscribe to the shrine, and a warning against using the building-fund (or the Delian contribution to it?) for any other shrine. The normal publication-formulae follow.

What is the occasion of this decree? The passage in *IG* i² 310 = i³ 383 is fair evidence for the existence || of an organised cult of Apollo Delios in 429/8. That our inscription is earlier than this, but not by much, is suggested by our observations of its cutting-style. One hardly has to look far for an occasion for the Athenians to appease Apollo Delios, and that appeasement may be in question is suggested by my restoration of line 3. If one sacrifices without saying why, the reason may be too ill-omened to mention. At the beginning of the Peloponnesian War, Thucydides (II.8.3) says ἔτι δὲ Δῆλος ἐκινήθη ὀλίγον πρὸ τούτων, πρότερον οὔπω σεισθεῖσα ἀφ' οὗ Ἕλληνες μέμνηνται· ἐλέγετο δὲ καὶ ἐδόκει ἐπὶ τοῖς μέλλουσι γενήσεσθαι σημῆναι. I would have thought that, if Thucydides says ὀλίγον, he means it, and it was probably within the year. Nor would anyone have thought anything else, had not Herodotus (IV.98.1) mentioned at some length an earthquake in Delos in 490 καὶ πρῶτα καὶ ὕστατα μεχρὶ ἐμεῦ. To straddle and suppose that there was only one earthquake, in 460, and that both Herodotus and Thucydides were foolishly mistaken, as How and Wells do, is hardly a satisfactory solution. The simplest solution is that Herodotus wrote this passage before 432,[14] but believed in a 490 earthquake. Thucydides for some reason did not and says so with customary indirectness.[15]

With the war close at hand, Apollo Delios needed appeasement and the state took over his cult at Phaleron. But with the Perikles–Kallias economy régime in force, the placatory shrine had to be a cheap one. Closer dating is hardly possible. If we are within 432/1, Antiochis can have held the first, second, or seventh prytanies,[16] but if I am right in allowing eight letters for the secretary's name, the first prytany is ruled out, since the first secretary of 432/1 had nine letters.[17] With this approximate date the proposer Lysikles may be Aspasia's husband, the general of 428/7, but this is still no more than a possibility.

[14] The view that this is a late stretch of Herodotus depends on an unnecessary interpretation of VI.91.1, for clearly the expulsion of the παχέες and not of all Aeginetans is in question there, and this may have happened long before 431.

[15] He need not be correcting Herodotus, but must, I think, be correcting somebody. Not so Gomme, ad loc. and *Essays*, 122 n. 2.

[16] Meritt, *Athenian Financial Documents* 71–89. [17] *Ibid.* line 3.

20

After the profanation of the Mysteries

In our generation there have been few who have made such a consistent and successful attempt to see Athenian life as a whole as the author of *The People of Aristophanes*. In gratitude to him I offer for his inspection an investigation of texts which have attracted his attention in the past. The main purpose of the investigation is to see in practice the workings of one part of the Athenian administrative system when faced with problems of unusual size and complexity.[1]

Our texts concern the aftermath of the mutilation of the Hermae and the profanation of the Mysteries. We need not retell here the story of the crimes and their investigation. Partly to serve as a framework, a few facts may be recalled. Within the archon-year 415/14, the mutilators of the Hermae were condemned to death, their names were put on stelai and their property was confiscated.[2] For the profanation of the Mysteries, Alcibiades was condemned to death, his property was confiscated, his name was put on a stele, and all priests and priestesses were instructed to curse him. All of them duly did so, turning to the west and waving red sheets, except one priestess who said she was a praying priestess, not a cursing priestess.[3] Most of these penalties can be reasonably extended to the other celebrators of the Mysteries. That all this is to be taken perfectly seriously is to be deduced from what happened when Alcibiades came back in 407. The stele on which the con-

* Published in *Ancient Society and Institutions . . . V. Ehrenberg* (1966), 177–91 (Basil Blackwell). The fragments of the 'Attic Stelai' are re-edited by Lewis as *IG* i^3 421–30.

[1] An earlier form of this paper was read in Philadelphia, New York and Toronto, in the winter of 1964/5, and I owe much to the hospitality and discussion I met with in these cities. That I am slightly more satisfied with this later version is due to the help and criticism of M. I. Finley.

[2] Philochoros, 328F134.

[3] Thuc. VI.61.7; Plut. *Alc.* 22.5, Nepos, *Alc.* 4.5. The details of the curse are from Lys. VI.51, but the extension to Alcibiades seems legitimate. I agree with Busolt, *GG*, iii. 2, 1319 n. 2, that only the Eleusinian priesthood was involved (cf. Thuc. VIII.53.2).

demnation was recorded was thrown into the sea, and the people formally requested that the priests withdraw their curse. (Again, there was an exception to make the stories symmetrical; this time the hierophant remarked diplomatically: 'If he is no wronger of the city, I never cursed him.')[4] Nor can we doubt that the armed pilgrimage to celebrate the Mysteries which Alcibiades undertook was to demonstrate that he and the city were once more at peace with the Goddesses;[5] calculation shows that this must have been virtually his last act before he sailed again, and he had wasted a great deal of good campaigning || weather to do this, while Lysander was rebuilding the Spartan fleet.[6] As we come to expose ourselves to much matter-of-fact detail, we should not forget that there lies behind it a major disturbance in Athenian religious life.

The title now usually, perhaps rather misleadingly, given to our texts, the 'Attic Stelai', is from Pollux,[7] who quotes a word from 'the Attic Stelai, which are situated at Eleusis and on which the property of those who were impious with regard to the Goddesses and were sold up by the state has been written up'. This statement caused trouble in the past and must now be regarded as a mistake. No single fragment of such a text has turned up at Eleusis, and, although the earliest known fragments came impartially from various parts of Athens, the Agora excavations have produced a great concentration of them at the south-east corner of the Agora, that is, in the area of the Eleusinion. Anne Pippin[8] has shown satisfactorily that Pollux got his information about these texts directly or indirectly from Eratosthenes and that somewhere along the line the Eleusinion turned into Eleusis. We can reserve Pollux's information that the Stelai were for the property of those who were impious with regard to the Goddesses for further investigation; we recall that the mutilators of the Hermae had their property confiscated too.

Let us turn to what we have. The Agora excavations enormously increased the body of these texts to the point where they needed a thorough organisation and publication, which they received in 1953 from W. K. Pritchett.[9]

[4] Diod. XIII.69.2, Plut. *Alc.* 33.3. [5] Xen. *Hell.* 1.4.20, Plut. *Alc.* 34.3–6.

[6] κατέσχεν αὐτὸν μεχρὶ μυστηρίων (Plut. 34.3) is practically a direct statement on this point. The return was on Thargelion 24–5 (Plut. 34.1), the Mysteries on Boedromion 19–22. τρίτῳ μηνὶ in Xen. *Hell.* 1.4.21 cannot be right. If Pritchett is right to detect the suppression of days in this period (*BCH* lxxxviii (1964), 455–73), they may not be irrelevant.

[7] X.36. [8] *Hesperia* 25 (1956), 318–25. [9] *Hesperia* 22 (1953), 225–99.

This was a major operation. Not many of the pieces are very large. Some of them, though obviously cut by the same stonecutter, equally obviously belong to more than one stele. The texts on the stones are of very little help. Entries are not continuous for more than a line or so, and the arrangement varies very considerably even inside a column. The cutters continually side-slip their entries to make room, both with columns of figures and with larger items of text. Pritchett has threaded his way through all this with great skill and has established the framework for future study of these texts; when he published some new fragments in 1961,[10] he was able to incorporate them without too much trouble. This essay inevitably builds on his foundations, with rather different interests. The commentary which he and Amyx have devoted to the texts[11] has concentrated on using them as quarries, on identifying the objects sold and using the evidence of the texts for their prices. There would still seem to be some room for thought about the texts themselves and the operation which they record.

These stelai must be classified as almost a unique class of documents, || recording the sale of one group, though a very large group, of confiscated property. Virtually all the other records of such sales that we know are incorporated in the annual records of the *poletai*, the public sellers. Almost unique, but are they quite unique? There are two other stones which seem to have recorded nothing but the sale of confiscated property, on the parts which survive nothing but real property.[12] They have been taken more or less for granted, but they are looking more and more isolated and abnormal. I am beginning to think that we should be looking for an occasion fairly soon after 403 BC when a large group of real property could have been confiscated. If this is right, the search may be very short. One of the stones has a subheading [τῶν] ἕνδεκα |[οἰ]κίαι ἃς οἱ δήμ[αρχοι ἀπέγραψαν].[13] Meritt[14] not unnaturally associated this with the evidence in the *Ath. Pol.*[15] that confiscated property could pass through the hands of the Eleven, but he was bothered by the fact that there seemed to be no room to restore [παρὰ τῶν] ἕνδεκα. It is tempting to give [τῶν] ἕνδεκα its natural meaning, 'the property

[10] *Hesperia* 30 (1961), 23–9. [11] *Hesperia* 25 (1956), 178–328; 27 (1958), 163–310.

[12] *IG* ii² 1579 + *Hesperia* 15 (1946), 181, no. 31 + *Hesperia* 16 (1947), 149, no. 38, *Hesperia* 5 (1936), 390, no. 9. [On the confiscations of 403 see M. B. Walbank, *Hesperia* 51 (1982), 74–98, cf. *SEG* xxxii 161.]

[13] *Hesperia* 15 (1946), 182, lines 30–2. [14] *Ibid.* 184.

[15] 52. 1. Cf. *Hesperia* 10 (1941), 14, lines 1–7.

of the Eleven'; such genitive headings are frequent in our stelai. We recall that the amnesty of 403 applied to all Athenians save the Thirty, the Ten, the Piraeus Ten and the Eleven;[16] we recall that the Athenians made processional implements from the confiscated property of the Thirty.[17] Looking at the other stele, we see one name, Drakontides, which is that of one of the Thirty.[18] We would certainly need at least one more coincidence of name before regarding the identification of these texts as in any way secure, but it suffices to note that our stelai may not be quite unique.

Pritchett has sorted our texts into ten stelai (I propose to ignore his Stele XI throughout, and I think I would have his support). The problem of how the Athenians organised texts on multiple stelai is one which is only just beginning to receive attention,[19] and it is certainly a serious one here. Only one of the stelai preserves any kind of heading. This is VII and most of its heading has been lost – some of it by recent damage. It seems to have read [τὸν π]ερὶ τὰ [μυστέρια ἀ]σεβεσ[άντον], and clearly ran across both columns of text, though Pritchett gives it all to column I. There were certainly two and possibly a third of the others which began *in mediis rebus*, that is II, X and I.[20] The beginnings of these stelai will not have made sense in isolation, and the reader will have to have been guided to them from another stele by their relative positions. It is therefore disappointing to find that apparently none of the stones has any trace on its edge to indicate that it made a close fit with another stone either to left or to right. The only indication of any kind which can be || brought into play here is precisely on Stele VII, the only one which is self-explanatory. This has in its top a small hole and a shallow pour-channel for molten lead. This pour-channel goes, surprisingly, from right to left. For it to make any sense at all, we have to visualise some kind of crowning member placed on the left-hand side of this stone and the right-hand side of another. Though there is no trace of any anathyrosis or smoothing

[16] *Ath. Pol.* 39.6. [17] Philochoros, 328F181.

[18] *Hesperia* 5 (1936), 392, lines 14–15; Xen. *Hell.* II.3.2.

[19] Most of the discussion has been verbal and arises from the facts assembled by Dow, *Hesperia* 30 (1961), 58–73. The group which I discussed briefly in *BSA* 49 (1954), 37, is also relevant.

[20] Stele I does not stand on the same level of probability as II and X, since the argument for placing its fragment *a* (*Hesperia* 22 (1953), 244) is rather fragile. It is difficult to repress some uneasiness about the reconstruction of Stele I. Note that the argument used for placing fragment *g* (*ibid.* 245) has been invalidated by the discovery of fragments *i* and *j* (*Hesperia* 30 (1961), 23–4).

either on the left edge or on top, the cutting is certainly classical: at least one pair of stelai had a crowning member.[21]

The stones have other interesting architectural features. Stelai I–III are all in the same hand, and all have the same thickness (0.114 m). We cannot precisely calculate the height or width of any of them, but Pritchett has argued that I and II at least could have been of the same dimensions. Stele II, more spectacularly, has been drastically thinned at the sides of the back, so that, at the edges and for some way in, the back is fairly tidy and only 0.075 m thick, whereas the centre remains thicker and hardly touched. The conclusion would seem to be that no one was intended to get round the back of the stones, which will have stood fairly close to a wall, but that they might pass by the sides, which therefore had their backs tidied as far as the eye might penetrate. We can imagine Stelai I–III, of the same thickness, standing in a line. Somehow linked to these is Stele IV, which has the same lettering and also has a smooth band at the back, but which is much thinner (0.08 m). It either stood further forward or round a corner. Thickness also links Stelai VI and VII (0.15 m), but there are serious difficulties about giving them the same dimensions. Stele VII only had two inscribed columns and was never much wider than its present half-metre, while Stele VI presents major problems, since we now have seventeen fragments, none of which has any original edge. It is not quite clear what Pritchett means by saying that he assembles the fragments on the assumption that the inscription was a very large block of marble, not properly a stele. The absence of original edges is not particularly surprising in a stone which has been re-used and re-cut, and it is not clear that the original dimensions need have been unreasonably large, particularly if we discard some fragments, as we may later see some slight reason to do. Admittedly, it will be larger than Stele VII, but it may have stood in line with it. We know the thickness of two other stelai, V (0.124–0.125) and X (0.095), but nothing else very helpful about them.

In the area of the Eleusinion, east of the temple, is a narrow rectangular monument base, 2 × 15 m, of which the subterranean parts || remain in poros. It is dated by the pottery to the second half of the fifth century.[22] It is not impossible that it has something to do with our texts. Unfortunately the

[21] I am grateful to Professor Homer A. Thompson for removing my suspicions of this cutting.

[22] *Hesperia* 29 (1960), 338.

excavators have not as yet been able to satisfy themselves of its relation to the peribolos wall of the whole sanctuary, and there is no real point in speculating as to whether the stelai could have gone on the base, when we do not even know whether the public could walk round the base. No other possible site for our stelai has yet appeared.

The result of all this for our further problems is that Pritchett's numerical order is to some extent a logical order from a purely epigraphic point of view. Arguments of a rather different kind put Stele X at or towards the end of the series. Uniquely, as far as we can tell, among our texts, it is arranged chronologically, by the dates on which the sales took place. The temptation to say that this is later refinement is strengthened by a consideration of the dates. Points put by Pritchett[23] exclude the stone from the year 415/14 and offer a nine-out-of-ten probability that it belongs to 414/13, more precisely to the early spring of 413. As one of the items sold[24] consists of crops on land in the Troad, and military conditions were not favourable to such an operation in any later spring than that of 413, the date seems practically certain. One would expect selling operations to have got under way rather earlier than that. Philochorus[25] and common sense put the confiscation in 415/14, but that does not necessarily date the actual sale. Pritchett[26] effectively uses some evidence in Stele II which included sales of crops of olives, figs and grapes. He says perfectly reasonably that the proper time for such sales should be September, and that therefore Stele II represents a transaction in some sense distinct in time from Stele X. He then has to choose a September for Stele II, which he generalizes into 'the main series of sales'. The choice is between September 415 and September 414. He does not actually make the choice, but he is clearly disturbed at the thought that the auction would last as long as eighteen months and appears to prefer to close the gap by putting Stele II in September 414. He then accounts for the delay before the auction starts, which would have to be a full year, by using Fine's view that selling land was a relatively new development in Attica and suggesting that the machinery for disposing of confiscated land might have taken some time to establish.

This is not the place to discuss the history of Athenian land-tenure, but this cannot, I believe, be the answer. I am myself satisfied that the Athenians

[23] *Hesperia* 22 (1953), 233. [24] Stele X, II. [25] See note 2.
[26] *Hesperia* 22 (1953), 233–4.

had been confiscating land and selling it for at least 140 years.[27] Even if we confine ourselves to unimpeachable evidence, || they had certainly been threatening confiscation as a penalty for twenty-five years[28] and it would be surprising if no land had ever been confiscated before 415. The sale of land may indeed have raised special problems, but I doubt if they arose out of the unfamiliarity of the idea of sale.

Let us step back for a moment and consider the problem which faced the Athenians. The literary sources, notably Andocides, 1, suggest that there were about fifty people whose estates had to be wound up. (Incidentally, our texts have hardly twenty names, though doubtless some of the property which they list belonged to others.) They might have had property of clear and ascertainable value: cash, gold and silver objects, money out on loan. Most of them will have had town houses, all of which had a certain quantity of furniture, cooking-utensils and other effects. Many of them had country properties, some of them, no doubt, several in different parts of Attica. To complicate the situation, some of these properties were out on lease, and the rent would still be coming in or need to be extracted. Worst of all, some of them had a remarkable amount of property overseas in various parts of the Athenian Empire, mostly in Euboea, but some was as far away as Thasos and the Hellespont.[29] Most of them had slaves on their properties and perhaps some living independently. A metic shows in our texts with at least 16,[30] and we have two men with 8 and 7.[31] At least one of these slaves, a shoemaker, had property in his own possession. Since it was legally his master's, it also had to be sold.[32]

On the problem of disposal, our texts show at least one thing clearly. The estates were certainly not taken one at a time, beginning, say, with Alcibiades,

[27] *Historia* 12 (1963), 37 with n. 136.

[28] *IG* i² 45. 24 = i³ 46. 28; cf. *IG* i² 10. 31 = i³ 14. 32; 39. 34 = i³ 40. 34, for even earlier threats against non-Athenians.

[29] The political and economic importance of investment in overseas land by Athenians is a much neglected topic. It is clear from our texts that the guarantees in the Second Athenian Confederacy (*IG* ii² 43 = Tod 123) against overseas land-holding by Athenians can have been directed against this pattern at least as much as against cleruchies.

[30] Stele 1, 34–49.

[31] Adeimantos, Stele VI, 18–23; 54; X, 3; Axiochos, Stele X, 7, 9 and *Hesperia* 30 (1961), 26.

[32] Stele VI, 31–46.

continuing with his uncle Axiochos, and so on. Even in our texts, property both of Axiochos and of Adeimantos, the demesman of Alcibiades and Axiochos, was sold on nine different occasions,[33] and three other people reach four entries. These multiple sales and the delays which they reveal must rest on two factors; the delay in knowing what had to be disposed of and the delay before the actual disposal, whether caused by the sheer load of work or by outside difficulties.

Our texts, it is generally and probably rightly agreed, were drawn up by the *poletai*. Their fourth-century function in this context is outlined by Aristotle:[34] ἀναγράφουσι δὲ καὶ τὰ χωρία καὶ τὰς οἰκίας, τὰ[πο]γρα[φ]έντα καὶ πραθέντα ἐν τῷ δικαστηρίῳ· καὶ γὰρ ταῦθ' οὗτοι πωλ[οῦσιν]. In our texts, they go beyond this to the extent that they sell property other than real property, but there is no reason to doubt that a denunciation (ἀπογραφή) and a court-decision || that the property denounced was in fact confiscated property had to precede sale. The later fourth-century texts are precise on this, giving the name of the court and of the denouncer.[35] The other point essential to the activity of the *poletai* is that they are *sellers*; only property capable of sale by auction will normally come within their purview. This means that a considerable amount of the property concerned ought not to appear in our texts at all. It is therefore not surprising that there is only one cash item[36] and one silver object[37] in the texts; the surprise is that they are there at all, and we ought not to exclude the possibility that we have misunderstood these items. It is hardly less surprising, on this line of reasoning, that receipts of rents are reported.[38] These items are a puzzle. Whether they record receipts from existing leases or – a theoretical possibility, nearer to one of the normal activities of the *poletai* – a lease pending sale of property already taken over by the state, they have on the face of it nothing to do with

[33] Axiochos: Stele II, new fragment (*Hesperia* 30 (1961), 26), II–17, IV, 10–12; VI, 118–21; new fragment (*Hesperia* 30, 28) 13; VII, 46ff.; (?) 62ff.; X, 6–8; 23; 30. Adeimantos: from before II, 161 to new fragment (*Hesperia* 30, 25–6) 10; VI. 17–29; 53–61; 116–17; 131–2; 174ff.; X, 3; 10; 25.

[34] *Ath. Pol.* 47.3. [35] See, e.g., *Hesperia* 5 (1936), 401, lines 115–20.

[36] Stele II, 182.

[37] Stele VI, 172–3, with Amyx's discussion (*Hesperia* 27 (1958), 206–8). I would think that we should in fact restore ποτέ[ριον ἀργυρῶν] | τορ[ευτόν] since the column is too narrow as it stands. The restoration in Stele VII, 93 has been rightly abandoned (*Hesperia* 25 (1956), 308–9).

[38] Stele II, 173–5 (the figure should surely be 𐅄 H 𐅄 Γ III); VI, 110ff.

an auction,[39] and perhaps cast some doubt on our belief that the texts were drawn up by the *poletai*.

If sales do not take place until after a denunciation and a court-decision, some delay is already accounted for. The *poletai* may have done something to look for property which would have to be sold, but no doubt the formal denunciations would be made by others. The stress on private initiative in Athenian law-enforcement allows the probability that, as in the fourth century, there were denunciations by private individuals, but there is clear evidence for at least one piece of quasi-official intervention, again normal later, in use for the first time that we know of:[40] denunciation by demarchs, the local officials who could be expected to know who owned what in their deme. In Stele v, 20–41 we find a patch of such denunciations recorded, and we can in fact fill in some restorations, for the only demarchs who would be denouncing property in Athmonon and Kerameis will be the demarchs of Athmonon and Kerameis.[41] It is possible too that other officials, state, not local, played a part. In the very early fourth century we find such denunciations being made by officials called ζητηταί.[42] This was an extraordinary office, not a regular one,[43] but, since it was used in 415 for investigating the charges against the Hermokopidai,[44] it would not be surprising if its functions were extended for this purpose.

There is delay then until the denunciations are made, until they are read out in the assembly,[45] until the court decision. Further delay could be caused with real property if there were claims on it. To take a case of 366 as a parallel, a piece of property was already known || to have one lien on it when it was confiscated. Before it was sold, three other claims of liens were made and had to be settled in court.[46] The technical name for such a claim was ἐνεπισκῆμμα; it appeared at least once in our texts.[47] If there were many of them, further delay would be inevitable. The overseas property might cause further difficulty. Besides the mere facts of distance and lack of information,

[39] Contrast the language of the document *IG* ii² 1590 + 1591 + new fragment (*Hesperia* 6 (1937), 454ff.) which certainly gives first leases by *poletai*.

[40] [Plut.] *X Or.* 834 A τὼ δὲ δημάρχω ἀποφῆναι τὴν οὐσίαν αὐτοῖν (of traitors in 411) is a close second.

[41] Lines 38 and 41 (where the three-obol sign must be the sales-tax from the next column) make it clear that 20, 23, 27 had nothing after ἀπέγραφε. Read [Ἀθμονέ]ων in 23 and [Κεραμέο]ν in 27. Other changes in these lines are collected in *Hesperia* 25 (1956), 317.

[42] Lys. XXI.16. [43] Dem. XXIV.11 and Harp. *s.v.* [44] And. I.14.

[45] *Ath. Pol.* 43.4. [46] *SEG* xii 100.8–39. [47] Stele IX, 8.

it was after all in another jurisdiction, and this might occasion further legal procedure, involving other officials, perhaps Athenian,[48] perhaps foreign.[49]

We should therefore expect to find in our texts a pattern of sales which would be hard to analyse, caused by the varying delays before denunciation, with an overriding tendency for real property and overseas sales to come late. On the other hand, prudence would suggest an early sale of property which might remove itself, like slaves, or which might easily be removed by others, or which might spoil.

Let us take a closer look at our texts.

Stele I contains a good deal which can certainly be attributed to Alcibiades, whose name appears at least once.[50] It consists of bronzes, blankets, clothes and furniture. We can certainly assume that they came from his town-house. We also have the list of sixteen or more slaves belonging to the metic in Piraeus, which seem to have formed a convenient group for disposal. There is one long list of stores – vinegar, wine, olive-oil and barley – which may be country or town, but look like kitchen-stores for a large establishment.[51] The only time we certainly go outside the immediate vicinity of the city on this stele is to sell off three lots of standing crops. At least in one of these cases[52] we are certainly dealing with an emergency measure, since all the property of that owner which is sold is one slave, perhaps the farm slave, and the crops, not a big item at that.

Stele II begins in a kitchen and certainly a very big one: Miss Pippin and Professor Amyx[53] are surely right to guess that it could be that of Alcibiades. After a break we are in a list of slaves – luxury slaves for the most part, including a goldsmith, a butler and an expensive Macedonian lady – and pass straight to another group of standing crops, two lots in places we cannot identify, one in Eretria. In column II, we are certainly on a farm, conducting a mass sale. The greater part of this column has no individual prices against the items. Though the objects were listed meticulously, they were sold as a whole. Eventually, we discover that we are in some farms of Adeimantos[54] and then dispose of five slaves of Axiochos. A new type of entry then ends

[48] A reference to *epimeletai* in Eretria looks attractive in Stele VI, 150–1.

[49] Admittedly the Athenians look to be acting pretty freely about property in another state in *IG* i² 101 = i³ 96. 5 (cf. *BSA* xlix (1954), 30).

[50] Line 12. [51] Lines 113–39. [52] Lines 26–32. The others are lines 20–3.

[53] *Hesperia* 25 (1956), 325; 27 (1958), 183f.

[54] The new fragment, *Hesperia* 30 (1961), 25–6, has to be incorporated here.

column II and begins column III; in it, it appears || that nothing is being sold, but the entries report receipts of rents and so on. At this point of column III, we seem to reach some kind of stop, perhaps the end of the activity of the board of 415/14. The new heading (184–6) can hardly be reconstructed with certainty, but it must have said in part 'This is the property of those condemned for their impiety about the Mysteries.' This however is what we have been dealing with all along, if we go by the names. Only one of the five preceding names, to go by Andocides, was concerned only with the Hermai and not with the Mysteries. A new start looks the most likely solution. Next comes what is clearly Phaidros' town-house, and beyond a relatively clear patch of farm equipment at Phaleron in column IV, Stele II becomes disappointing. It will be noticed that so far no single piece of real property has been sold. This may be an accident of survival, but certainly the impression we have been given so far is that movables have been disposed of, but as yet no house or land. Only on the very last fragment assigned to Stele II is there any land, and this is land overseas in Euboea, the largest single item of our texts, selling for 81 talents 2,000 drachmae. Pritchett was already very uncertain as to whether this fragment belonged to this stele; it now looks as if it may be as anomalous textually and administratively as it is epigraphically.

About Stele III we can say nothing, save that it has a small patch of kitchen-furniture. Stele IV is quite different in arrangement and content. It is not arranged by owners but by category. The large fragment preserves the end of a list of houses belonging to at least three different owners and the beginning of a list of overseas property. Somewhere else on the stone, a good deal of property of Alcibiades was sold; real property, I suspect. On Stele V, however, we are back with a text which has no sales of real property; it seems to contain mostly things to be found on farms. I imagine that Pritchett only put Stele IV before Stele V because it is in the same hand as Stelai I–III, but it would be rash to assume that, once the *poletai* started selling real property, they never sold anything else, and thus reverse the order of Stelai IV–V. The chief interest of Stele V is in the passage about demarchs already referred to.

Much of the arrangement of Stele VI cannot be recovered. It is arranged by individuals, but there is a good deal of real property in it. To confine ourselves to the patch where the arrangement is certain,[55] we find that the process of selling up Adeimantos has reached as far as Thasos, but various other

[55] Lines 50–122.

pieces of land quite near the city are still being sold. Some rents are still coming in. Virtually everything on this stele is closely connected with land, save perhaps for one difficult || passage.[56] The apparent exception at lines 168–70 can certainly be abolished. Pritchett could make little of κεττ[ο]ί, except by comparing Hesych. κησσόν· εὔοδμον, which leads to no good sense, particularly in view of the price, 110 drachmae, if not much more. Read Κεττ[ο]ῖ, the locative of the deme Kettos.[57] This leaves on the stele only things intimately connected with land, like cattle, bees, jars of wine and slaves.

The current restorations of Stele VII, column i, start with a list of slaves with no owner named. This is not inescapable, since an owner's name could be substituted for τάδε τὰ in line 2, but since lines 37 and 82 certainly had simply κεφάλαιον οἰκιῶν, with no owner's name, and a personal heading appears at line 46, the stele certainly had a mixture of category-classification and personal classification. At least in one place (line 78) we are as far away as Abydos.

There is nothing very much to be said about Stelai VIII–IX. Stele X is the stele with dates, and seems to show that proceedings have slowed down enormously. One slave on Gamelion 7, another two weeks later, then three days later some crops in the Troad and four pieces of real property sold for very little. Pritchett[58] is puzzled by these low prices, on which two points should be made. Firstly, these are sales by auction, which can make for deceptive prices. Just as two incautious buyers in an auction-room can produce an extremely high price, very low prices can be realised if there is only one buyer and no reserve. There is a second possibility. We are after all in late winter 414/13 and the threat of renewed hostilities and *epiteichismos* may be affecting the prices of outlying properties. In any case, it is clear that the ownership of these slaves and land has only just been established.[59]

The hypothetical picture sketched before has on the whole been borne out by this analysis of the texts. To be more precise, the *poletai* began with

[56] Line 86. I do not care much for ἐκχαλ[κόματα] (Tod's correction of the spelling). In view of Nikides' Euboean interests (IV, 17–8) perhaps ἐκ Χαλ[κίδος ἐπικαρπία] *vel sim.*

[57] π[(line 168) might be e.g. π[ιτυϊνόν] (cf. X, 1) πρόσ[κ]λ[ιντρον] in line 169 will also go (the photograph makes *kappa* very dubious); πρός with a place-name?

[58] *Hesperia* 25 (1956), 275.

[59] In X, 23 the restoration κεφ[άλαιον τούτον] has had too long a run. It makes no sense, for there is nothing to sum, and the spacing is quite wrong for it (see the photograph, *Hesperia* 22 (1953), Pl. 83). Κεφ[is a slave's name, as the price (195 drachmae) makes clear, and the sales-tax [Ͱ Ͱ] should be restored on the left.

the furniture and what they could find in town houses. They then moved out to the country and cleared off the movables there too. It was only after their operations had gone some way that they began to dispose of the real property itself. If we consider their problems, particularly the overriding one of their dependence on denunciations in order to find out what they had to sell, it will not seem surprising that the whole operation took eighteen months or so. I would call it rather good going.

But even if we are going to compliment the Athenians on that score, I am not disposed to go much further, and I would like to consider briefly some points which bother me. Firstly, what do these stelai represent? Pollux said that they were those of the property of those || who were impious to the Goddesses, and this makes good sense of their being put up in the Eleusinion. Hermes had nothing to do with the Eleusinion. However, complications would arise immediately with those involved in both cases, and we can see these complications being felt. On Stele X, a distinction is made between sales on the same day of property belonging to Adeimantos, who was only implicated in the Mysteries, and of property τõμ περὶ ἀμφότερα. We can, I think, tell that the *poletai* woke up to this distinction rather late. The phrase περὶ ἀμφότερα appears twice on Stele VI. Pritchett noticed that on both occasions its lettering was rather crowded.[60] We can go a stage farther than that, for, if we think these words away, we can see that what remains is spaced absolutely normally;[61] it seems quite certain that these words were added later on Stele VI, when someone decided that this was a desirable distinction to make. It is not clear whether we should expect to find a heading τõμ περὶ τὸς Ἔρμας ἀσεβεσάντον on these stelai in the Eleusinion; it has not yet appeared.

Why make the distinction? Not purely to keep the historical record straight, for that would be made clear by the separate stelai which are attested, almost certainly of bronze, on which the sentences were inscribed. The alternative is to suppose some half-hearted attempt at accounting here. It is hard to see what kind of accounting. The normal formula for confiscation was that the property went to the state and Athena got a tenth. I can find no trace of variation in this procedure,[62] but it does not seem entirely equitable or logical that Athena should get a tenth in a case where she was not among the deities

[60] VI, 89; 94; *Hesperia* 22 (1953), 277. [61] *Hesperia* 22 (1953), Pl. 78.

[62] Save perhaps for the mysterious πεντηκοσταὶ τῶν ἄλλων θεῶν (Dem. XXIV.120).

directly affected. One would have expected Hermes and the Goddesses to benefit to some extent, and making a distinction between the relevant property might help in this respect. However, when we next come to a datable set of accounts for the Goddesses in 408,[63] there is no trace whatever of a windfall of this kind, and it is hard to see what they can have spent it on in the interval.[64]

In fact, it is hard to see how anyone can have done any accounting from our texts. Attempts to add up figures are sporadic and inefficient. There are variations even in these additions, depending on how the sales-taxes are dealt with. The practice in Stele I is to add up everything under a person's name and write κεφάλαιον σὺν ἐπονίοις or κεφάλαιον of somebody, and this, roughly, applies to II, VIII, IX and X as well. Stele VII follows a different practice with three totals, κεφάλαιον οἰκιῶν, κεφάλαιον ἐπονίον, and σύμπαν κεφάλαιον. Stele VI has no totals whatever in practically all its fragments, so that I am even disposed to try to use this as a new way to classify || fragments and to put a question-mark against fragments *k* and *n* of Stele VI, which do have κεφάλαιον.

Totals there must have been somewhere, and I think that they must have been rather considerable. By the fourth century, confiscations were almost a recognised, though irregular, part of Athenian budgeting. By 396 or so, it is possible to argue against excessive public spending on the grounds that, when the budget is balancing, the council looks after the city properly, but, when it is running into debt, it becomes much more ready to accept accusations which will result in confiscations.[65] The contribution of our operation to Athenian finance in a difficult couple of years will have been considerable. What we have on the stones comes to over 100 talents. This is swollen by one 81 talents entry which happens to have survived, and I do not suppose this was unique. We are after all dealing with fifty or so Athenians of the highest social class. I would say that 500 talents was an extremely conservative estimate of their total capital value, and I would guess that the real answer was somewhat closer to 1,000 talents. In other words, the figure was of an order close to the annual yield of the tribute of the empire in this period, and must have considerably lightened the burden of the Sicilian expedition.

[63] *IG* i² 313 = i³ 386.

[64] Hermes had some money available in 409/8 (*IG* i² 301. 12, 69 = i³ 376. 13, 71), but we cannot tell how much.

[65] Lys. XXX.22.

Two points however have to be considered before we accept this in our picture of Athenian finance. First, there is one clear statement that private denouncers took three-quarters of the property they denounced.[66] There is not very much obvious supporting evidence,[67] though there are clear cases of enthusiasm in denouncing confiscated property.[68] I can only record a feeling that this seems a very high proportion for our kind of case. If it was at all normal for three-quarters of confiscated property to go to the denouncer and a tenth to Athena, the alleged attraction of confiscation for a financially embarrassed council has obviously been much exaggerated. We have no means of telling what proportion of denunciations were made by private persons and what by demarchs or ζητηταί. Secondly, fourth-century law allowed the spreading of payments for houses over five years and for land over ten.[69] There is evidence that lump sums were nevertheless paid,[70] but in the texts which we have suggested belong to the Thirty and the Eleven, the partial down-payments are normal.[71] It is therefore possible that not all the proceeds of our sales came in at once.

A word or two of later history. When Alcibiades came back to Athens in 407, the Athenians voted to drop the stelai on which his condemnation was written in the sea. Those, I am sure, were bronze || stelai, nothing to do with our texts. These remained standing. Eratosthenes was able to know of them and their contents in the third century and the evidence suggests that they lasted much longer than that. No single fragment of them has appeared even in a Roman archaeological context, and I suspect that they lasted into the Byzantine period. There was also a vote in 407 to restore Alcibiades' property. One might well imagine that that would be difficult. It seems to have been commuted into a grant of land, and his son lost that too after the restoration of the democracy.[72]

[66] Dem. LIII.2, τὰ μὲν τρία μέρη, ἃ ἐκ τῶν νόμων τῷ ἰδιώτῃ τῷ ἀπογράψαντι γίγνεται, τῇ πόλει ἀφίημι. Cf. *IG* ii² 1631. 365–8 for a more indefinite statement of this reward.

[67] It is tempting to think that the otherwise mysterious phrase τ]ῆι πόλει τὰ τρίτα μέρη in *Hesperia* 19 (1950), 237, no. 14, line 42 might be restored as the record of a renunciation parallel to that in Dem. LIII.2. If τρίτα were then read in both places, the difficulties I feel would be greatly reduced.

[68] Lys. XVII.4. [69] *Ath. Pol.* 47.3.

[70] E.g. *Hesperia* 5 (1936), p. 402, lines 152; 185.

[71] This was first established by Meritt, *Hesperia* 4 (1935), 570f. [72] Isocr. XVI.46.

21

Aristophanes and politics

Aristophanes is unquestionably news. The *Lysistrata* is drawing its crowds in Sloane Square, in an admirable production,[1] which certainly doesn't merit its description as a 'travesty, emanating, I believe, from Oxford', given to me by a desiccated Cambridge don, though it may be thought that certain of its deeper elements have perhaps necessarily been played down. The translations of Messrs Dickinson and Fitts into English and American hurry alternately from the presses, while, if neither form of Standard English meets your taste, three shillings and sixpence sent to Mr Douglas Young of St Andrews will produce a translation into Lallans of the *Puddocks*, enlivened with references to Frankie Vaughan and Mr Kruschev, and enriched with learned notes and references to Liddell–Scott–Jones–Mackenzie and their 'muckle lexicon'.[2] It therefore seemed appropriate, when a paper was needed on this occasion, to resuscitate for your benefit a paper which I gave in France a year ago. I have no other motive, except possibly to start a fight among the undue number of learned attendants at this meeting. I don't think it possible to cover my ground in a short time, and I am uneasily aware of the traces that will show through my paper of the efforts to be elegant, Gallic and not shock the French too much which formed a large part of the original version.

* This paper, revised from a French version given in Nancy and Strasbourg in 1956, was read to the Oxford Branch of the Classical Association in 1957. The subject has been much studied since then: Lewis's paper is still worth reading, and it has not seemed appropriate to encumber it with forty years of subsequent bibliography.

[1] In 1957 the production by Minos Volanakis was staged first at the Playhouse, Oxford (cf., e.g., *The Times*, 13 March 1957, 3) and then at the Royal Court Theatre, London (cf., e.g., *The Times*, 27 December 1957, 9).

[2] *Aristophanes Against War* (*The Acharnians, The Peace, Lysistrata*) translated by P. Dickinson (Oxford University Press, 1957); subsequently *Aristophanes: Plays* translated by P. Dickinson (2 volumes: Oxford University Press, 1970). *Lysistrata / The Frogs / The Birds / Ladies' Day* (*Thesmophoriazusae*) translated by D. Fitts (New York: Harcourt, Brace, 1954/5/7/9; London: Faber, 1955/7/8/60). *The Puddocks* (*Frogs*) / *The Burdies* in Scots by D. Young (Tayport: the author, 1957, ²1958 / 1959).

I have had to change things. There is of course no call here to begin, as I did in France, with a description of what Mods is and what Greats is and how they differ. Everybody knows in Oxford that Mods is Mods and Greats is Greats.[3] In fact, it has been maintained that people have far too clear an idea of this distinction. Tacitus has ceased to be a Mods special book, Thucydides VI–VII hang on just, assisted by the fact that the hard-pressed history tutor finds it simpler to omit them in order to make sure in that crowded last term the essential job of understanding *Ath. Pol.* 30–1 in all their intricacies is not omitted. The pupil who feels he ought to study the Sicilian episode more carefully can always be crushed with the relevant quotation from Philistus.[4] Aristophanes is uncomfortably placed. In Mods he holds an assured place. The choruses are certainly literature and there is plenty of scope in the rest for the study of language. From this point of view there is plenty of work to do and a succession of Mods lecturers have done it honourably and well. In Greats the situation is more awkward. One wants in the fifth century to put a lot of weight on Thucydides and the inscriptions. They are serious and historical, but one feels one ought to be keeping an eye on Aristophanes the whole time. Old Comedy is full of the names of Pericles, Cleon, Alcibiades and Hyperbolus; the poets seem to be concerned with the great questions of war and peace. There ought to be here a mass of historical facts (and attitudes are also facts) which will add a great deal to our more conventional sources. Thucydides gives us a great deal, but, as we know, there are many things that he was unwilling to tell us, perhaps even things that he didn't know. He left Athens in 424, and we have seven plays of Aristophanes produced between that year and 404, when Thucydides got home. We should perhaps then be studying the plays in order to deepen our knowledge both of historical facts in the purest, most knobbly sense, and of the Athenian spirit. You will remember that, when Dionysius of Syracuse asked Plato to send him a book on the πολιτεία of the Athenians, Plato sent him a copy of Aristophanes. That, at any rate, is what one scholiast says. Another, perhaps rather better informed, says that Plato only sent him a copy of the *Clouds*, and, when one thinks about Plato's attitude to the *Clouds*,

[3] When this paper was written Mods, the first part of the Oxford BA course in Classics (Literae Humaniores), was devoted to Greek and Latin languages and literature, while Greats, the second part of the course, was devoted to ancient history and ancient and modern philosophy.

[4] Lewis was probably thinking of *FGrH* 556F56.

one wonders whether this isn't merely just one more example of Platonic εἰρωνεία.[5] But I think the principle is a good one. Let us try to find the Athenian πολιτεία in the plays of Aristophanes.

But there is no prospect of us achieving this tonight, if at all, for a preliminary problem must be faced at once. If Aristophanes knew that this was what we were doing, would he be in any way pleased about it, would he think this was a legitimate approach? The answer depends on our opinion about the aims of comedy, and one can hold widely diverse opinions about this. In the *Symposium*, Socrates is made to say, and Aristophanes does not demur, Ἀριστοφάνης, ᾧ περὶ Διόνυσον καὶ Ἀφροδίτην πᾶσα ἡ διατριβή (177E1–2). 'Drink and sex' is not an adequate translation. Sex may be all right for Aphrodite, but Dionysus, you would agree, is a good deal more than drink. He is the comic spirit, the transformation of everyday values into uncontrollable exaltation. If this definition of Aristophanes' διατριβή is accepted, there is no point in going much further. Nothing can be taken seriously, and the historian conducts his investigation at his own risk, with only one reason for confidence. We have no Aristophanes who can do justice to his lack of a sense of humour. But we can go for another definition to Aristophanes himself. In the *Frogs*, Aeschylus asks Euripides τίνος οὕνεκα χρὴ θαυμάζειν ἄνδρα ποητήν; 'for what qualities should one admire a poet?' Euripides replies, and Aeschylus accepts his reply, 'For his cleverness and his advice, and because we make the men of the city better' (1008–10). And at the end of the play Dionysus returns to the same theme: 'I have come down to look for a poet. Why? So that the city may be saved and keep the choruses going' (1418–19). The contest between Aeschylus and Euripides is not decided, finally, on artistic considerations, but only on the question, which of them will give the city the best advice. Now, you can of course say that all this only concerns tragic poets, but in the *Acharnians* the chorus has some spirited remarks on the same subject, though the lines have a great deal of comic colouring (628–58). The King of Persia asks the Spartan embassy whether it is the Spartans or the Athenians who have the slanderous poet. Whichever possesses him, he says, will be πολὺ βελτίους (647–51). And all these lines of self-praise in the *Acharnians* say nothing of his comic fertility, nothing of his

[5] In W. J. W. Koster's edition of the scholia, vol. 1a, xxviii.46–8 and xxix.a.33–5 both mention the *Clouds*; but R. Kassel and C. Austin, *Poetae Comici Graeci* III.ii, printing xxviii as T1, follow Bergk in deleting the reference to *Clouds* (42–5). Thanks are due to Dr R. B. Rutherford for help at this point.

lyric beauties. Aristophanes claims only that he is the βέλτιστα διδάσκων (658), and there are plenty of other passages of the same kind, right down to the *parabasis* of the *Frogs*, which has no comic colouring whatever:[6]

τὸν ἱερὸν χόρον δίκαιόν ἐστι χρηστὰ τῇ πόλει
ξυμπαραινεῖν καὶ διδάσκειν. (686–7)

Many scholars have followed these allusions and have given us their views on Aristophanes the politician, Aristophanes the propagandist. It is well known that most of the poets of Old Comedy permitted themselves to speak rather lightly of the most important leaders of the democracy. Couat thought that this tendency had a very simple explanation: the *choregoi*, who paid for the plays, were certainly rich and therefore certainly anti-democratic.[7] Croiset seems to have wavered between the view that Aristophanes was the spokesman of an agricultural party and the view that he was giving the opinion of a small circle of his friends.[8] Murray, and no one would want to accuse Murray of a want of sensibility about literary and artistic values, seemed frequently to see Aristophanes as primarily a propagandist for pacifism and Panhellenism.[9] And this last, of course, as far as one can judge, forms a large part of the current view of the man in the street. No one has protested against the title of Mr Dickinson's *Aristophanes against War*,[10] and most reviewers of the *Lysistrata*[11] have taken it for granted that Aristophanes generally had a political aim in mind. The case for Aristophanes the politician has been developed in detail at various points, to the degree that the *Acharnians* has been regarded as a manifesto for the election of *strategoi* in 425, spoilt at the last moment by the elections being held unexpectedly early.[12]

To all these opinions we can oppose the useful corrective of Gomme's article in *Classical Review* 1938.[13] For Gomme, Aristophanes is simply a dramatist. As a dramatist, he must understand and sympathise with everybody

[6] Ar. *Frogs* 686–705, cf. 718–37. [7] Couat, *Aristophane et l'ancienne comédie attique.*

[8] Croiset, *Aristophane et les partis à Athènes = Aristophanes and the Political Parties at Athens.*

[9] Murray, *Aristophanes: A Study.* [10] Cf. n. 2, above. [11] Cf. n. 1, above.

[12] Intended to influence elections of 425: A. B. West, *CP* 19 (1924), 124–46 + 201–28 at 205–10. Intended to influence elections but elections moved to an earlier date: H. B. Mayor, *JHS* 59 (1939), 45–64 at 55–63 (combined with the mistaken view that the generals entered office not at the new year but immediately after their election).

[13] *CR* 52 (1938), 97–109 = his *More Essays* 70–91.

if he is going to give us a faithful representation of his character. Gomme shows that sometimes we find those of his characters who are in the wrong are more sympathetic than those who are in the right. He might have adapted the dictum of the authors of *1066 and All That* on the Cavaliers and Roundheads and claimed, for example, that, in the *Wasps*, Bdelycleon is right but repulsive, while Philocleon is wrong but romantic or, perhaps, rather attractive. There is no question that Bdelycleon is in the right and Aristophanes gives him the convincing arguments, but he doesn't give him life or humanity. Secondly, Gomme shows that most of the generalisations one can make about Aristophanes' opinions can be nullified by a closer look at the text. Far from being a spokesman of the farmers, Aristophanes sometimes shows lively sympathy with the sailors. He is thought to have been a lover of the past to whom the old ways and the old books were the best. But every old man in Aristophanes is in the wrong. The old jurymen in the *Wasps* are partisans of Cleon. Strepsiades in the *Clouds* is not made to look anything but foolish. The old men in the *Lysistrata* want war to the end. More important, perhaps, this lover of the past or, if you prefer Croiset, of the conservative farmer, never stops praising himself for his novelties of style which have replaced the uncultured work of his predecessors. As to the view that Aristophanes wants peace above all and all the time, how, asks Gomme, can you explain that in the *Knights* there is scarcely any mention of peace and that the chorus is allowed songs of a distinctly patriotic and bellicose nature?

So far, so good, but Gomme goes further. For him, it is absolutely useless to look for the views of Aristophanes himself. We must only think of the plays that he has left us. If one knew the opinions of Aristophanes on political matters, this would have a certain interest, but wouldn't help us to understand the plays in any way. And this apparently works in reverse; there is no means of finding out what Aristophanes really thought from his plays. Of course, Gomme admits the existence of the passages where Aristophanes claims to be a teacher, but he thinks that perhaps they shouldn't be taken too seriously or that perhaps Aristophanes had more confidence in himself when he was young and thought that part of his duty as a poet consisted in giving political advice. I have already said that the most sustained and serious piece of advice comes in the *Frogs*, twenty years after the *Acharnians*. I can only conclude that the analysis given by Gomme, and I hope I have been fair to it, breaks down at this point, and, healthy, useful and

influential though it has been, it is high time we started looking for a safe position somewhere in the middle.

Before we look, I think we must define our position about the plays as plays. What does it mean to call them plays? And here I think we come to the central weakness in Gomme's position. He insists that Aristophanes is a dramatist, that he is holding a mirror up to life, portraying character, and takes his modern examples and parallels from Shaw, presumably because there too there is political content. I can only say that I think Gomme is deceiving himself with this definition of dramatist, and that the times when Aristophanes was trying to do anything of the kind are relatively few. All parallels between literary forms in different cultures are liable to be misleading, and I must say I consider the Shaw parallel to be a good deal more misleading than most. My basic guide-line, and I use this as an indication rather than a firm framework, is by now almost a commonplace, since it has even appeared in Oxford scholarship exam papers. Professor Dover, in *Fifty Years of Classical Scholarship*, introduced the radio programme *Take It From Here* into the discussion to illustrate his point about 'concentration on the comic moment', his thesis being that 'in Old Comedy the moment and the episode were everything, all consistency of place, time, character, personality and logic could be sacrificed to them'.[14] Let us take a little further the comparison with wireless comedy. No present series, including *Take It From Here*, seems to me nearly as close to Aristophanes as Tommy Handley's *ITMA*. Like Aristophanes essentially a war product, it was admired by the intellectuals, the middle and the working classes. Each week, the hero, as if in a picaresque novel, found himself, whether willingly or unwillingly, in a new situation, perhaps on a desert island, perhaps at Hitler's headquarters. Frequently he created the situation himself, by inventing fantastic projects to reorganise circumstances which he disliked. Given the situation, the results would follow. When one sets one's scene firmly in abnormality, it is the normal which produces the laughs. Of course, the dramatic construction was by no means rigid, and there was plenty of Professor Dover's temporary concentration on the comic moment as well as the general comic situation. Apart from all this, there were episodes which we might describe as lyrical, where the element of parody, never completely absent, had full scope.

[14] Platnauer (ed.), *Fifty Years of Classical Scholarship* 96–129 at 98 = *Fifty Years (and Twelve)* 123–58 at 125 = Dover, *Greek and the Greeks* 190–219 at 192.

Famous tunes of the past were thoroughly jazzed up; popular songs were handed over to full symphony orchestra and became the themes of fugues.[15]

There is plenty that is Aristophanic here, but what about the political allusions? With the BBC, of course, they tended, and tend, to exist in rather a general character, naming no names, concentrating on giving us a typical minister or civil servant. Modern cases of personal insult have to be looked for in the cartoonists, and the differences are not so great as you might think. Just as in Old Comedy Pericles has always his deformed head and Cleonymus has always just dropped his shield, a modern cartoonist must inevitably exaggerate Sir Winston's cigar, Mr Macmillan's moustache and M. Mendès-France's bottle of milk (I am sorry; that slipped in from the French version).

I think that these modern parallels make it easier to understand Aristophanes' aims. But modern parallels don't explain everything. One must remember that Aristophanes worked in a tradition, and it gives him nearly indispensable elements, the *agon*, the *komos* and *gamos*, which underline the sacred and orgiastic character of the festival, and, above all the *parabasis*, where the chorus speaks for the author. And one should never forget, though it is not my subject this evening, the calibre of Aristophanes as a lyric poet.

Perhaps we had better get back to the subject. Given the framework, what about the politics in it? Let us start from a contemporary's view. The Old Oligarch tells us 'The Athenians do not permit fun to be made of the *demos*, so that they may not be insulted themselves. But they encourage the poet to make fun of individuals, because they know that no one will make fun of a true man of the people, but rather of the rich, the noble or the powerful. Very few poor men or democrats appear in comedy, and only those of them who are too active and wish to raise themselves above the *demos*. They are not disturbed to see these men becoming objects of ridicule' ([Xen.] *Ath. Pol.* 2.18). You can see that, if Couat was right to say that the poets were the spokesmen of the rich, the Old Oligarch hadn't noticed it. What he does say seems to me very reasonable. The comedian wants to make his audience laugh, and one can make fun of prominent men much

[15] A note by Lewis, dated 1993, states, 'It should be noted that the most accessible tapes of *ITMA* (BBC Radio Collection (1988) ZBBC 1011) simply suppress these musical interludes.'

more easily than of nonentities. Prominent men, powerful men. It will often be the same thing. The comedian will almost always be 'agin the government'. In a democracy, it is the highest placed who are the best targets for the poet's arrows. And the audience enjoys it. If Cleon was a true man of the people, which I don't believe, he was, quite certainly, too active and wishing to raise himself above the *demos*. People have been surprised that the *demos* could give first prize and applause to the poet who insulted Cleon and then give important duties to the man insulted. It is hardly surprising really. Comedy-time is festival-time and all is topsy-turvy. Even in Oxford, we used to give an important place at Encaenia to the *Terrae Filius* and allow him to insult the most important figures of the University and the State.[16]

If that sketch is complete, we are still not too far from Gomme. But it can't be complete. We can't deny that the comic poet may have had personal opinions which he wanted to express or which he couldn't suppress. We can even name one poet, Hermippus, whose views on Pericles were so strong that he took them from the theatre into the courts.[17] And it could happen, even in the theatre, that the poet would go too far, either through lack of prudence or because the object of his attacks was rather more touchy than was suspected. We certainly know of two, perhaps three, cases of this kind. From 440 to 437, there were restrictions of some mysterious kind on the liberty of comic poets (schol. Ar. *Ach.* 67), and I think we must believe that the poets had expressed themselves with too little tact on some person or question. In 426 we have the well-known attack by Cleon on Aristophanes for having insulted the city in the presence of foreigners (*Ach.* 377–8, 502–3, with schol. 503). And then there is the law of 415, but goodness knows what that is about (schol. *Birds* 1297, schol. Aristid. iii. 444 Dindorf).[18] It seems to me quite certain that people sometimes took the poets more seriously than Gomme would like to believe. Some people, of course, couldn't see the point of the institution at any time, and Isocrates is at a loss to understand how

[16] At the Act from which Oxford's present-day Encaenia is derived the *Terrae Filius* was a licensed jester who delivered a scurrilous oration: the last reliably attested instance is that of 1713 (cf., e.g., V. H. H. Green in L. S. Sutherland and L. G. Mitchell (eds.), *The History of the University of Oxford* v (Oxford University Press, 1986), 351–2).

[17] Plut. *Per.* 32. 1. But notice the scepticism of K. J. Dover, Τάλαντα 8 (1975), 24–54 at 27–8 = his *The Greeks and their Legacy* 135–58 at 138; R. W. Wallace in Boegehold and Scafuro (eds.), *Athenian Identity and Civic Ideology* 127–55 at 131–2.

[18] For restrictions on comedy see recently S. Halliwell, *JHS* 111 (1991), 48–70, J. E. Atkinson, *CQ* n.s. 42 (1992), 56–64, who cite earlier discussions.

comic poets get praised for spreading insults to the city abroad. I suspect that he is speaking of the comic poets in his youth, even though he is now in his eighties. I can't believe that anything got Isocrates to a comedy much after the age of twenty-five.

If people sometimes took the poet seriously, I think we must allow him some political views of some seriousness which sometimes showed through, and I propose to sketch briefly where in Aristophanes they seem to me to show through. It will be a little easier if we go backwards. Even so, this won't be complete. I want to exclude the *Ecclesiazusae* and the *Ploutos*, which belong to another age when, as one of my audience has said, 'not only the poet, but Κωμῳδία herself' (and I would add the audience) 'has grown old and become worn out, and sceptical.'[19] They have their political interest, but not for this evening. I must, I am afraid, also rule out the *Thesmophoriazusae*, because its political allusions and chronological relationship to the oligarchic movement of 411 raise considerable difficulties, and I don't want to pick an unnecessarily detailed quarrel with my chairman.[20] But it seems to me that in the other two plays of the Decelean War, we can see fairly clearly. In the *Lysistrata*, it seems to me that Aristophanes starts by imagining the extremely comic results which would follow a sexual strike by the women. This is a purely comic idea, but one which the poet's heart deepens until he attains what I think is his highest point, pure and true pathos, an entirely serious expression of all that war means to women. Over and above that, I tried to show in a recent article that Lysistrata herself is only a portrait, scarcely disguised, of Lysimache, priestess of Athena Polias, and that there are in the play elements of the greatest possible seriousness.[21] Lysistrata is the spokesman of the ancestral religion of Athens and of all that is best in Athenian life. As such, she asks the men to abandon the foolishness of war, reminds the Athenians and the Spartans of the benefits they brought each other in the past, demands that the Greeks remember their common shrines and make peace. Nothing could be more serious, even though the exigencies of the original comic idea make it necessary that the hearers should yield to their need for their women rather than Lysistrata's arguments. It is quite impossible to believe that Aristophanes' real opinions cannot be seen here.

Six years later in the *Frogs* we have another problem. One can't call it a

[19] E. Fraenkel, *Greek Poetry and Life . . . G. Murray* 257–76 at 276.

[20] A. Andrewes, for whose views see Gomme *et al.*, *Hist. Comm. Thuc.* v.184–93.

[21] *BSA* 50 (1955), 1–12 = this volume, 187–202.

political comedy, but I've already said that its *parabasis* contains line after line of political advice without a word which could possibly be thought of as a joke, and that when the *agon* between Aeschylus and Euripides nears its end, the needs of the city become the subject of their contest. There is scarcely a word of peace anywhere, and one may believe that Aristophanes had given up hope of a negotiated peace. There is nothing in sight but the continuation of the struggle, and the poet contents himself with counselling political harmony and the pardon of those who went astray during the oligarchy of 411. He doesn't defend the oligarchy, but suggests that those whose mistakes had cost them civic rights should be given the chance to rehabilitate themselves. He adds that the city should improve its choice of leaders and give more confidence to men who are well-born and well brought up that it refuses to elect now. I think that the practical effect emerges if one considers the opinions which Euripides and Aeschylus give later on Alcibiades. Euripides opposes his return, Aeschylus, who speaks second, favours it (1420–34). If one remembers the words of the *parabasis*, it seems clear to me that it is with Aeschylus that Aristophanes agrees, although he will not go so far as to say so distinctly. In any case, whatever one thinks about this bit, I cannot conceive how Gomme believes that the audience laughed or was meant to laugh at the line 'What do you think about Alcibiades?' any more than I can see an audience laughing at his modern parallels 'What should we do about road-accidents?' or 'What should we do about Spain?' Another campaign must see the end of Athens or her victory, and there is no experienced general left. The question of Alcibiades is not a laughing matter, and Aristophanes doesn't treat it as such. But he gives no clear answer to his question, except in the generalised words of the *parabasis*. We are told that the play was given a second performance because of the *parabasis* (and no one, I hope, is going to tell me that we ought to read κατάβασις).[22] This may be so, but the advice of the *parabasis* was not followed.

I leave on one side the *Birds*, the most universal of all the plays, with the least roots in the circumstances of its times. We are often told that this rootlessness, this search for another world that is found in it, reflects the disgust Aristophanes felt for the world in which he was, his disgust at the follies

[22] *Frogs*, *hypothesis* i, *fin.* κατάβασιν for παράβασιν was proposed by Weil and accepted by Coulon; the suggestion is not mentioned in Dover's *apparatus*.

of the Melian massacre and the Sicilian expedition. I have no arguments against this view, and would merely remark that the poet has detached himself from these events so successfully that there is no possible means of proving it. For our enquiry, too, I don't think there is much profit to be got out of the *Peace*. Produced as it was at the Dionysia of 421, when everybody knew that the oaths for the Peace of Nikias would be sworn immediately after the festival, it could not be anything other than it was, a joyful masque to celebrate the return of peace to an exhausted Greece. One can't deny that Aristophanes welcomed the peace of 421, but who opposed it? Some months were to pass before its disadvantages appeared. All Aristophanes wants to do is to make his contribution to the general rejoicings. With some regrets, I pass even more quickly over the *Clouds* and the *Wasps*, labelling them as comedies of humours rather than political comedies to excuse my slackness, and arrive at last at the two great political comedies, the *Acharnians* and the *Knights*.

Nearly everybody is at one in considering the *Acharnians* as a passionate appeal for peace. Dikaiopolis, we are told, in whom we must recognise the voice, and perhaps even the face, of the poet himself gives a convincing demonstration of the advantages of peace, and I don't think that there is a more widespread view about Athenian party politics than that which regards it as certain that Aristophanes and the farmers wanted peace in 425.[23] I am sorry to say that I regard this view as completely untenable. If it was true that everybody knew that the farmers wanted peace, it is more than a little difficult to understand why those typical farmers, the Acharnians, show themselves so keen on the war at the beginning of the play. In fact, they appear roughly as we should have expected them to. They last appeared in the pages of history six years ago (Thuc. II.21), anxious to get at the Spartans who were ravaging their fields. Six years of war have only embittered them; nothing is more probable. If these bellicose old men were in real life vigorous supporters of peace, their yielding to the arguments of Dikaiopolis would make nonsense of the comedy, if I may be permitted a figure of speech which I propose to christen the oxy-oxy. It seems to me more reasonable to see in this reversal of attitude a purely comic mechanism. Dikaiopolis convinces the most bellicose of all Athenians with arguments

[23] This view is not as widespread now as it was when Lewis wrote. See, as one instance among many, W. G. Forrest, *Phoen.* 17 (1963), 1–12 (Lewis noted in 1993 that, as far as he could recall, there was no cross-fertilisation between his paper and Forrest's).

about the start of the war so frivolous that it needed the special talents of Ephorus, a provincial trained in the interpretation of Attic comedy by that great expert Isocrates, to find a place for them in serious history. And what, we may ask, are the aims of Dikaiopolis, on the orthodox view? Surely he wants to bring peace to the whole city, the whole of Greece? Unfortunately, this is not so. He wants peace for himself, personally and solely. When he gets it, he hoards it like a miser and will give none of it to a farmer who has suffered from the war nor to his starving neighbours, and the triumphal end of the play does not show us the benefits that peace brings to the city, but only the personal satisfaction of Dikaiopolis, who can take his ease and drink his fill, while his neighbour Lamachos has to face the painful duties of serving his country.

It may nevertheless be objected that this is the way Aristophanes uses to show the Athenians the benefits that a general peace would bring. He doesn't say so, and, if one considers the *parabasis*, where one would hope to hear some more specific advice, one finds no indication that Aristophanes thinks the war may end soon. He tells us that the Spartans want peace, on condition that the Athenians give Aegina back her independence. 'Don't you give it to them', says the poet; 'if you give up Aegina, you will lose your poet' (652–5). (Aristophanes seems to have had a farm there.[24]) Now this is mostly fun, and I wouldn't dream of interpreting it as a serious warning against a serious peace-movement, but it does seem to count the other way and to fit extremely badly with the popular portrait of the propagandist who wants peace at any price. I must say that I can't see the *Acharnians* as anything but an attempt to work out the results of a comic idea. The festival is coming and Aristophanes must write a play. He looks for a comic idea, a new situation which will give opportunities for producing laughter. He says to himself, 'If an individual tried to make peace by himself, a peace from which everybody else was excluded, wouldn't that be a good subject for a comedy?' He was quite right, and I see no reason to suspect anything more ambitious in the *Acharnians*.

The author, or authors, of the *Knights*, as even Gomme admits, disliked Cleon, and the play seems to me to suffer from a confusion of aims. The

[24] On Aristophanes and Aegina see Figueira, *Athens and Aigina in the Age of Imperial Colonization* 79–93; but notice the argument of D. M. MacDowell, *CQ* n.s. 32 (1982), 21–6 at 24, that it was Aristophanes' producer Callistratus who was connected with Aegina.

comic idea which ought to have filled the play is simple: what would happen, if one put against Cleon a demagogue who was even more violent and even less scrupulous? And most of the play is in fact devoted to the comic elaboration of this theme. The struggle is conducted on a relatively low level, though one can, I think, draw some conclusions about the real Cleon from it.[25] But there are passages where the Sausage-Seller stops talking like a demagogue. At the end, you remember, he takes Demos back to the golden age of Marathon and Salamis and makes him once more worthy of his heritage. A splendid end, but one quite incompatible with nearly everything that has gone before. There is another earlier passage of about thirty lines, where the Sausage-Seller suddenly stops his flattery of Demos and denounces Cleon in the most serious language as being responsible for the continuation of the war in order to avoid the discovery of his crimes (790–822). Immediately afterwards, he goes back to a catalogue of frivolous and absurd accusations. It is not, I think, the Sausage-Seller who is speaking here, it is the poet who cannot restrain his personal opinions. But even if one admits that the poet thought in 424 that, after the victory of Pylos, the war had become useless, there is still no reason to assume that he wrote the *Knights* as a politician who wanted to attain a political end. And among modern theories there are some which do not satisfy me at all. The poet who wrote the choruses of the *Knights* was not a convinced anti-militarist on principle, and the poet who hails the rejuvenated Demos as 'King of the Hellenes' (1333), 'monarch of Greece' (1330), was not in 424 either a Panhellenist or a champion of equal rights for the allies.

Do these investigations of individual plays lead to anything which can be called a result? Does a formula emerge which expresses the amount of political content and aim which may be looked for in Aristophanes? I don't think it does. The opinions of the poet can be seen here and there, but one can't trace every step of his political development. This does not worry me, and here at last I must side with Gomme, if one has to come down one side or other of the playwright-politician dichotomy. One would expect to be able to trace the stages in the political thought of a propagandist, but a playwright and poet has other things to give us, although among the other things, as we have seen, one can from time to time catch sight of the opinions of a patriotic and honest man of broad sympathies. This is a sketchy account, I

[25] Cf. Lewis's review of the edition by A. H. Sommerstein, *CR* n.s. 33 (1983), 175–7.

know, and a thorough investigation would take much longer. The difficulties are great. One can always be accused of a lack of sense of humour, and quite frequently one can in fact lack it. Above all, one must try to understand Aristophanes and Athens for themselves, and, if one uses such aids and parallels as the standards of a corrupt French journalist, a pamphlet of the United Nations Association or a weekly radio programme, one must be ready to drop them as soon as they fail to fit the facts, and not try to assimilate the plays too completely to whatever modern standard we are using. With this flexible approach, one's views will shift. My own have changed from year to year, sometimes from day to day, and on each rereading I find something new. Every generation and every reader will have his own Aristophanes, and rightly so. But, if the investigation is difficult, that does not mean that it is not worth the trouble. Whoever gets closest to sharing the emotions and laughter of the original Aristophanic audience will also be the nearest to understanding the true nature of fifth-century Athens, and this after all is one of the main reasons for learning Greek and forming Classical Associations.

22

Who was Lysistrata?

Papademetriou, in publishing a new grave-inscription[1] from Zographo (see Plate 2), has propounded a theory which, if true, has important consequences for our understanding of Attic comedy in general and of the *Lysistrata* of Aristophanes in particular. For convenience I reprint the text with two slight modifications.

Καλλιμάχο θυγατρὸς τηλαυγὲς μνῆμα ⟨τόδ' ἐστιν,⟩
ἣ πρώτη Νίκης ἀμφεπόλευσε νεών.
εὐλογίαι δ' ὄνομ' ἔσχε συνέμπορον, ὡς ἀπὸ θείας
Μυρρίν⟨η ἐ⟩κλήθη συντυχίας ἐτύμως·
πρώτε Ἀθηναίας Νίκες ἕδος ἀμφεπόλευσεν
ἐκ πάντων κλήρωι Μυρρίνη εὐτυχίαι.

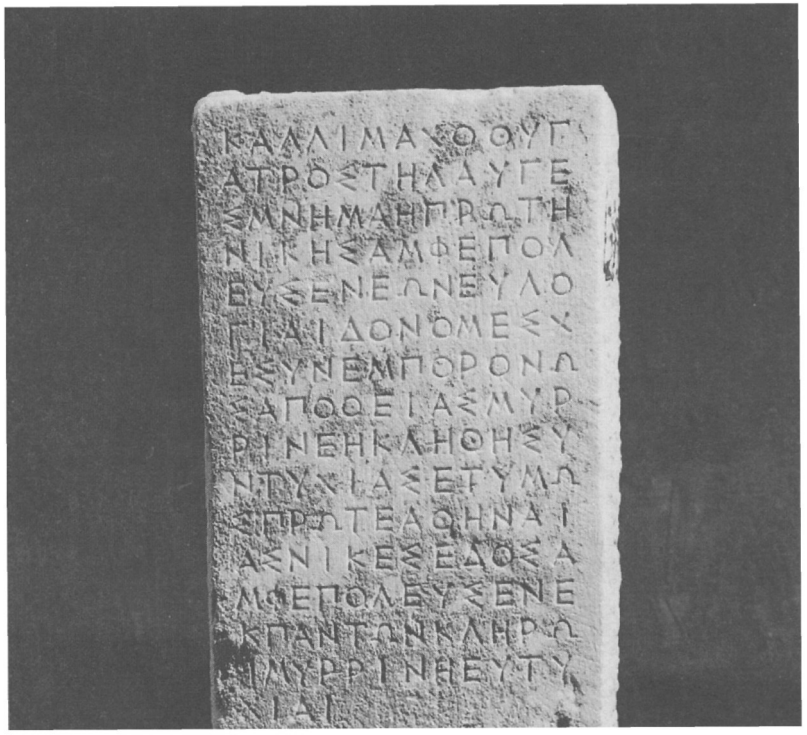
In line 4 the stone has ΜΥΡΡΙΝΕΗΚΛΗΘΗ. I prefer to assume that the stone-cutter, uncertain of the correct use of *eta* (cf. πρώτε and Νίκες in line 5), has transposed the letters, rather than assume with Papademetriou an unnecessary and unparalleled lengthened form and an omitted augment. After ἐτύμως the three dots of punctuation are clear. Even if they were not, metre and sense would suggest that ἐτύμως went with the previous sentence. ἐτύμως has nearly all its later meaning here. Papademetriou has illustrated the cult-significance of the name Μυρρίνη.

His thesis is this. We know the first priestess of Athena Nike was appointed round about 450,[2] perhaps after the Peace of Kallias. Here is her gravestone, which, from the style of its lettering and the transition to Ionic which it illustrates, ought to be close to 400. Therefore in 411, when the

* Published as the first item (XXIII in the continuous numbering) in Lewis's 'Notes on Attic Inscriptions, II', in *BSA* 50 (1955), 1–12. Lewis thanked A. Andrewes and many others for help with these notes.

[1] Ἀρχ. Ἐφ. 1948/9, 146ff. (*SEG* xii 80); [*IG* i³ 1330, with further bibliography].

[2] *IG* i² 24 = i³ 35 (ML 44).



2 The epitaph of Myrrhine: *IG* i³ 1330 (photo courtesy of the Epigraphic Museum, Athens).

Lysistrata was performed, the Priestess of Athena Nike was called Myrrhine. In the *Lysistrata* a Myrrhine appears at the Propylaea, perhaps even on the Nike Bastion, and appears to have a bed and other equipment close at hand. We should therefore assume that Aristophanes' Myrrhine is meant as a portrait.

We bristle with questions. Was the stage-setting really so elaborate as to make the || position of the Nike Bastion clear? Was Myrrhine's husband really called Kinesias, or has Aristophanes selected the name for its meaning and/or paired Myrrhine with an old antagonist? Is the inscription really as late as 400? Ionic had been coming into private inscriptions for half a century. The inscription could be as early as 420, and since Myrrhine is one of the commonest names in Attica, the case for the identification would disappear.

If the thesis is to stand, it must find further support. Papademetriou realises that Myrrhine cannot stand alone. If there is one genuine priestess, there must be more, and at least one must outrank her. Lysistrata is clearly no ordinary woman. At the beginning of the play, she has ordered the women of Greece to assemble and is surprised that they have not come at her command. Even before the assembly of women has decided to strike against the war and the tyranny of man, the elder women have, on her instructions, seized the Acropolis || under pretence of a sacrifice. Throughout the play her authority is unquestioned by women and recognised, at first grudgingly, at last willingly, even by men. Finally, when the scene moves to the Acropolis, she is completely at home there, and knows every part of it and all that happens in it with certainty. If Myrrhine is a real woman and Priestess of Athena Nike, there is only one position in the state that Lysistrata can hold. She will have to be the Priestess of Athena Polias, the servant of the ancestral cult of the olive-wood statue of Athena, which, at the moment of the play, was resting in a temporary shrine, while the Erechtheum builders planned the completion of their much-interrupted operations.

There is still no need for Lysistrata to be a priestess. She does nothing which could not easily be attributed to any comic hero or heroine. But before we come to the facts and see whether there is anything which tends to prove her priesthood, it may be profitable to ask whether our understanding of the play is at all deepened if we assume it. I feel that this is certainly so. One relevant note appears to be struck in the very first lines of the play. 'If they were summoned', she says, 'to a feast of Bacchus or Pan, of Aphrodite Kolias or Genetyllis, one wouldn't be able to move for the tambourines. But now no single woman has come.' Here I think we see the representative of the austerity of the old cult, and her distaste for flamboyance in religion comes out again later in a contemptuous reference to Corybantes. A little later the representative from Sparta arrives, a healthy, strapping creature. The women of Athens, who did not take much exercise, prod her with curiosity, led by Lysistrata. The Spartan girl complains that she is being handled as if she were a sacrificial animal.[3] If she were surrounded by Athenian priestesses, this would have a point which the audience could hardly miss. But this is a triviality. What is important, I think, is that Lysistrata is meant to represent the oldest and best elements in Athenian

[3] Aristophanes, *Lys.* 84.

life, which, if properly emphasised, will reject war as the foolish thing that it is. This the Proboulos, arriving to put down the revolt, does not at first realise. Hearing that the women have revolted, he assumes that they have done so under the influence of Adonis or some such new-fangled god, and in fact repeats[4] the word 'tambourines' which Lysistrata rejected in the first lines of the play. He is soon disillusioned. The women have revolted, not because they are the most unstable, but because they are the most stable element in Athenian society. As the chorus says,[5] 'When I was seven, I was *arrephoros*. At ten I made the cake for Athena's offering, and then wore the saffron to be a bear for Artemis of Brauron. And once as a fair young girl, I was *kanephoros*, carried the basket, and wore the necklace of figs. Have I not the right to advise the city?' They, the women, have been closest to the religion which holds Athens together, and Lysistrata, as the spokesman of that religion, calls for unity in Athens and all Greece. She reminds[6] the Greeks of their common festivals at Olympia, Delphi, and Thermopylae, and as the representative of historical continuity reminds the chorus of old men that there is another side to the events that they had recalled in their early choruses. They sang of the heroes of the age of Athenian expansion,[7] but they also, not only the Spartans, had forgotten that in that age Athens had been proud to come to the aid of Sparta, struck by earthquake and besieged by her own slaves.[8] They remembered how Cleomenes of Sparta had been driven off the Acropolis by the newborn democracy in 507,[9] but had forgotten the beginning of that story, and how Cleomenes had destroyed the tyranny of the Peisistratids for them.[10] Lysistrata, the one wholly serious character in the play, holds it together and gives it its peculiar force. I think that not a little point is added if we think of Aristophanes putting his message in the mouth of the priestess of the most important state cult. ||

The theory therefore has its seductions. Are there any facts which may give it plausibility? The priesthood of Athena Polias, like the priesthood of Poseidon Erechtheus, was a preserve of the Eteoboutadai, though the two priesthoods seem to have belonged to different families inside the *genos*. Now it so happens that the branch which supplied the priestess of Polias had a habit of giving names to its members very like Lysistrate. Admittedly the first certain member with that name is a remote descendant in the

[4] *Ibid.* 488. [5] *Ibid.* 641ff. [6] *Ibid.* 1130ff.
[7] *Ibid.* 801ff. [8] *Ibid.* 1137ff. [9] *Ibid.* 274ff.
[10] *Ibid.* 1149ff.

female line in the first century BC,[11] but it is generally considered likely that the priestess of Polias praised in a decree of 255 BC was called Lysistrate.[12] She seems to have been preceded in office by her aunt, whose name was almost certainly the very similar one of Lysimache. 'Dissolver of armies' and 'Dissolver of battle' come to very much the same thing. Lysimache's father, who was born between 400 and 371, possibly about 375, was called, quite certainly, Lysistratos, so that we can see that, even if it is not her real name, Lysistrate is a very suitable name, not only for a peacemaker, but also for a priestess of Athena Polias.

There is, however, evidence, I think, to show that it was not quite her real name. Plutarch[13] tells of a famous joke made by a Lysimache who was priestess of Athena. This could, of course, refer to the Lysimache of the beginning of the third century, but there is evidence for another Lysimache of an earlier date. The elder Pliny tells us[14] *Demetrius Lysimachen (fecit) quae sacerdos Minervae fuit LXIIII annis.* It is generally agreed that this is the Demetrios of Alopeke of whose work Lucian[15] gives us a vivid description. He was considered an innovator in naturalistic portraiture, and for this reason some have tried to date him rather low in the fourth century.[16] But the facts are clear. All his four signed bases belong to the first half of the century on the evidence of their letter-forms, supported in one case by an identification of the subject,[17] and it does not seem to me likely that any of them belong after 360. He made a statue of Simon, the author of the first manual of horsemanship,[18] who is referred to by Aristophanes as early as 424[19] and is unlikely to have died much after 385. The statue described by Lucian is of a Corinthian general, Pellichos.[20] He is not himself known, but it will be remembered that one of the Corinthian generals at Sybota in 433 was Aristeus the son of Pellichos.[21] It seems not unlikely that this is his son. If we date

[11] *IG* ii² 1036.34. [12] For this and the rest of the paragraph, see Appendix.

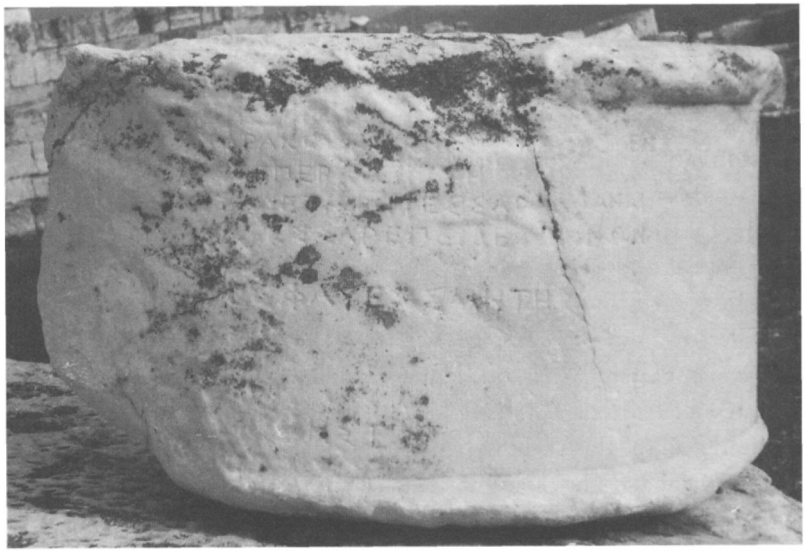
[13] *Vit. Pud.* 534 c. [14] *N.H.* xxxiv.76. [15] *Philopseudes* 18–20.

[16] Pfuhl, *Die Anfänge der griechischen Bildniskunst* 7.

[17] *IG* ii² 3828, 4321 (revised Meritt, *Hesperia* 16 (1947), 288), 4322, 4895. The son of the Hippolochides who is commemorated in 4895 was trierarch before 334/3 BC (ii² 1623.26), probably in 337/6 BC. The Kephisodotos of Aithalidai of 3828 may be the one who appears in the mining records (Hopper, *BSA* 48 (1953), 245, collects the evidence), and the date in the first half of the century would again be confirmed, but there are two men who differ only in patronymic (*PA* 8321, 8323) and *PA* 8322 may be a third. The second-century base, *Pergamon* 142, also belongs (Carpenter, *AJA* 58 (1954), 5).

[18] Pliny, *NH.* xxxiv.76; cf. Xen. *De re equestr.* i. 1. [19] *Eq.* 242ff. [20] See n. 15.

[21] Thuc. 1.29.2.



3 The base of Demetrios' Lysimache: *IG* ii² 3453.

Demetrios' working life from 400 to 360, we contradict none of the available evidence.[22]

The dating of sculptors is seldom convincing, and there is stronger ground for dating Demetrios' portrait of Lysimache. Benndorf[23] pointed out long ago that there is a strong || possibility that we still possess its base. To the west of the Parthenon towards the south wall of the Acropolis is a circular base[24] which once held a standing bronze of about two-thirds life size. I reprint the preserved text. See Plate 3.

[22] The dates have been pushed even higher, e.g. by Picard, *Manuel d'archéologie grecque: La Sculpture* iii. 126ff., and, very tentatively, on epigraphic grounds, by Raubitschek (*Dedications from the Athenian Akropolis* no. 143, p. 159) and Meritt, *Hesperia* 17 (1948), 38ff., no. 24. But Raubitschek warns us specifically (488) that his D143 is so uncertain as to have no probative value. His photographs hardly do justice to the probability that its two pieces belong together, which seems more likely when the actual stones are examined. He places it in the last quarter of the fifth century. I would not myself have put it much later than 425, and one would perhaps expect a sculptor to be in a position to dedicate for himself later rather than earlier in his career. Meritt's new fragment only has]πεκεθεν ἐπόησεν with nothing of the name, and must in any case be close to 400.

[23] *AM* 7 (1882), 47. The objections of Toepffer (*Attische Genealogie* 128) are irrelevant since the identification of ii² 3464.

[24] *IG* ii² 3453.

] Δρακο[. . . .]ο ἦν [. . 5 . .]ος μέν,
ἐ]ξεπέρασεν ἔτη·
-κον]τα δ' ἔτη [κ]αὶ τέσσαρ[α] Ἀθήναι,
τ]έσσαρ' ἐπεῖδε τέκνων.
]έος Φλυέως μήτηρ.
ἐπό]ησεν.

The name of the person commemorated and the sculptor's name have both perished, but there are certain points to tie it to our Lysimache. To take Benndorf's points first, the woman commemorated lived to a great age and did something for Athena over a period of years ending in four. This corresponds well to Pliny's description. She was the mother of someone from the deme of Phlya. Toepffer[25] demonstrated that the gentile qualification for priesthood was transmissible in the female line, and in the late Hellenistic and Roman periods priestesses appear in other demes than that of Bate, where we first find them. The first deme so to appear is Phlya,[26] and if the subject of our base, who married into the deme of Phlya, was of the priestly family, this is a satisfactory explanation of another priestess from Phlya in the first half of the second century. To Benndorf's points I can add no certain epigraphic evidence that the base is of a statue by Demetrios. None of the four signatures of Demetrios is in the || same hand as our base, but they are all in different hands, and, whatever may have been the case with sculptors in stone, there is no particular reason to suppose that a bronze-worker like Demetrios employed any regular stone-cutter to carve his bases. One prosopographical point can, however, be added. It has, since Benndorf's time, been realised that the name of the father of this lady must be Drakontides. Reisch suggested that he was the general of 433.[27] The only attraction of this identification is a limited one. It would have a certain piquancy if Demetrios made statues of the children of two of the opposing generals at Sybota. Stahl,[28] however, made it very probable that the deme of the general of 433 was Thorai, and there is no particular reason to suppose that he was of the priestly family. Another Drakontides is known, the father of the secretary of the Treasurers of Athena for 416/15.[29] Not only did he certainly come from Bate, but his son's name Lysikles shows the characteristic family root. I think it very probable that our base did carry Demetrios' bronze of

[25] *Attische Genealogie* 125ff. [26] See Appendix, no. 6.
[27] *ÖJh* 19–20 (1919), 304. [28] *RM* 40 (1885), 439. [29] *PA* 4549.

Lysimache, and that she was the daughter of Drakontides of Bate and the sister of Lysikles.[30]

The exact date of the base hardly matters. Kirchner[31] placed it around 360. This seems to me to be a little late. It appears to be in the same hand and from the same workshop as the troublesome unsigned base *IG* ii² 3091, for which opinion seems to be tending to a date in the early seventies.[32] Whatever the precise date, there would seem to be a case for maintaining that Lysimache was the sister of a man born before 446, that her sixty-four years as priestess ended some time between 390 and 360 and that she was therefore priestess of Athena Polias in 411 when the *Lysistrata* was performed. It will be seen from the Appendix that even if we manipulate the Demetrios evidence and ignore the base, the latest possible date for her to have entered office is still 405.

If this is so, I do not see how we can ignore the possibility that Lysistrata was deliberately modelled on her. As we have said, from Lysimache to Lysistrate is not a long jump. If a Lysimache was priestess of Athena Polias, the audience must have had the gravest suspicions when they saw a Lysistrate of great authority among men and women, closely associated with the Acropolis and possibly even with the priestess of Athena Nike. The mask may well have made all things clear. The possibility of coincidence would be further reduced with the lines:[33]

> ἀλλ' ἥνπερ ὅ τε γλυκύθυμος Ἔρως χἡ Κυπρογένεί' Ἀφροδίτη
> ἵμερον ἡμῶν κατὰ τῶν κόλπων καὶ τῶν μηρῶν καταπνεύσῃ,
> κᾆτ' ἐντήξῃ τέτανον τερπνὸν τοῖς ἀνδράσι καὶ ῥοπαλισμούς,
> οἵμαί ποτε Λυσιμάχας ἡμᾶς ἐν τοῖς Ἕλλησι καλεῖσθαι.

[30] I note for the sake of completeness that a statue has been found for the base by Six (*RM* 27 (1872), 83ff.), *viz.* Brit. Mus. 2001, a head of Roman date, and a striking portrait of an old woman which commended itself for the purpose by the aggressive naturalism of her face coupled with the fifth-century nature of her hair. Professor J. M. G. Toynbee has been good enough to confirm that the original was probably a bronze and that the hair appears to be of fifth-century style, but she doubts whether the face could have become nearly as realistic as it now is much before the end of the second century. Therefore, even if we could demonstrate that its prototype is the work of Demetrios, we would still be far from an idea of the bronze original.

[31] *Ad IG* ii² 3453.

[32] Pickard-Cambridge, *Dramatic Festivals of Athens* 52ff. = 2nd edn 54ff.

[33] Aristophanes, *Lys.* 551ff.

'If the sweetness of Love and Aphrodite of Cyprus aid us and we are successful in our plot, I think that the Greeks will call us "Dissolvers of battle"' or 'The Greeks will call us Lysimaches.' Would not the audience now be sure that she has been called Lysimache all the time?

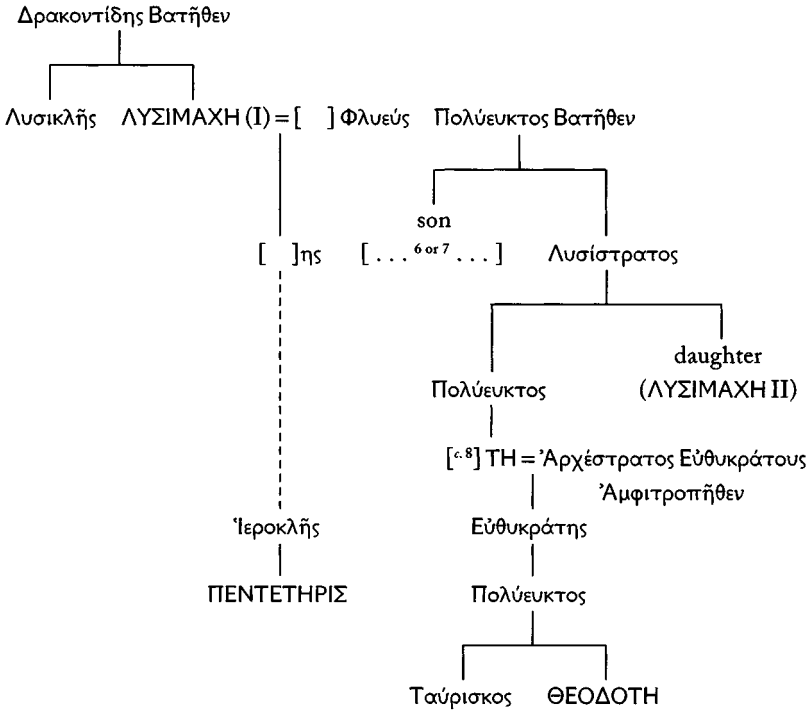
If my epigraphic and historical case is sound, I do not see how the audience could have avoided making the identification. If so, Aristophanes must have courted it, for he could || easily have selected a name with the same meaning, but without the close resemblance. Παυσιστράτη comes to mind immediately.[34] It may be disturbing to find that we have missed this strand in the play's meaning, but now that we have the evidence, it can hardly be denied that it is as relevant to our study of the play in relation to its time as our knowledge of the war-situation of 411.

New problems, of course, emerge, but I leave them to others better qualified to deal with them. I have sketched some of them in connection with Myrrhine. One may speculate why Lysimache's name was veiled and Myrrhine's not. Who was Kalonike and how was she Lysistrata's κωμῆτις?[35] Did the Proboulos indicate which of the ten he was by speech, mask, or gesture? The old view that Lysistrate is a coined name has long gone out,[36] but there is no evidence yet for it among the Eteoboutadai in the fifth century. Were they so proud of the play that they took the name over later?

[34] *Cf.* e.g. *IG* ii² 1529.10. [35] Aristophanes *Lys.* 5.

[36] Schmid, *Geschichte der griechischen Literatur* iv. 2. 1, 206 n. 11. The name is now known before the middle of the fifth century (*SEG* x 321 = *IG* i³ 953) and *c.* 410 (Beazley, *AJA* 54 (1950), 319). I have nothing to add to Beazley's remarks on the Jatta vase bearing the names Lysistrate and Myrrhine.

APPENDIX
The priestesses of Athena Polias



Stemma of priestesses of Athena Polias from the fifth to the second century (Priestesses in capitals).

The latest list of priestesses of Athena Polias is in Toepffer, *Attische Genealogie*, 128ff. It is now incomplete, and it may be found useful if I revise it here. The chronological order cannot be guaranteed at all points.

(1) Λυσιμάχη (I) Δρακοντίδου Βατῆεν is fully discussed in the main text. ||

(2) Φανοστράτη Ἀνακο[. . . . . c. 14 . . . .] θυγάτηρ. Priestess in 341/0 BC. *IG* ii² 1456. 31ff. Ἀνακο[is a new reading in line 31. The restoration of lines 34ff. can be improved, and note should be taken of the right margin. ἐν δὲ [τεῖ | ἀριστερᾶι χειρὶ κι]βώτιον χαλκοῦ[ν ἔ|χει, ὃ ἀνέθηκεν ἡ ἱερέα] Φανοστράτη [Ἀν|ακο . . . . . c. 14 . . . . θ]υγάτηρ. The name beginning with Ἀνακο[, presumably to be connected with the

Anakes, is unknown, although Bechtel (*Historische Personennamen* 44) has considered the possibility that a name like Anakodoros might exist, and speculation as to how this priestess fits the family cannot even start. It would be attractive, of course, to suppose a slip by the mason and read ⟨Δρ⟩ακο[ντίδου Βατῆθεν], which would fit the space well enough, but the possibility is not worth pursuing. [The *Lexicon of Greek Personal Names* ii knows only Ἀνάκων (*SEG* xxi 779).]

(3) Λυσιμάχη (II) Λυσιστράτου Βατῆθεν. As will be seen from my *stemma*, I have returned to Kirchner's original *stemma* (*ad PA* 9615), rejecting that suggested by him *ad IG* ii² 776. The new *stemma* was never very satisfactory in that it transmitted the name Polyeuktos from great grandfather to great grandson instead of from grandfather to grandson, and in that it posited a Λυσίστρατος Λυσιστράτου for whom there was no evidence. As it rested on dates for the archons Olbios and Alcibiades which are now regarded as much too low (I follow Pritchett and Meritt, *Chronology of Hellenistic Athens*), there is no reason to accept it. I list the evidence for the family group which includes Priestesses 3 and 4. [Pritchett and Meritt dated Alcibiades 255/4, and most later studies have placed him then or slightly earlier, but Habicht, *Untersuchungen zur politischen Geschichte Athens* 142 with n. 145, 144, prefers a date in the late 230s.]

Λυσίστρατος Πολυεύκτου Βατῆθεν, *prytanis* 341/0 (*IG* ii² 1749. 34), and therefore born before 371.

His brother [. . $^{6-7}$ . .] Πολυεύκτου Βατῆθεν (ii² 5867), died *c.* 340. His name may be the missing link to the earlier group.

His daughter, priestess of Athena Polias, [----Λυ]σιστρά[του] Βατῆθεν (ii² 3455), whose statue was made by the sons of Praxiteles. The base survives, but its lettering does not, as may be seen from the photograph in Marcadé, *Recueil des signatures de sculpteurs grecs* i. 58.

His son, Πολύευκτος Λυσιστράτου Βατῆθεν, archon of the Mesogeioi in the archonship of Oloios (277/6) (ii² 1245).

His granddaughter [. . . $^{c.8}$ . . .]τη Πολυεύκτου Βατῆθεν (ii² 776), priestess of Athena Polias in the archonship of Alcibiades (255/4? cf. above), married to Ἀρχέστρατος Εὐθυκρ[άτους Ἀμφιτροπῆθεν].

Λυσιμάχη, presumably priestess of Athena Polias, mentioned in the base of her servant Sueris set up in the second quarter of the third century (ii² 3464; Paus. 1.27.4). Reisch (*ÖJh* 19–20 (1919), 299) maintained that Lysimache I was in question here, and that for some reason the base had to be recut eighty years or so after it was set up. This is not happy, and his further reasons carry no conviction. He could not believe in two priestesses called Lysimache for reasons which would count against any repetition of names in any family, and appealed to a date in the 360s for the sculptor Nikomachos. This has to rest on a rejection of Koehler's contention that

IG ii² 3038 was twice used and bears two distinct inscriptions and on a doubtful restoration of line 1 of that inscription. Mr. O. S. G. Crawford was good enough to take an excellent photograph of 3038 for me, and after some study of it and of the stone itself (from a distance), I am reasonably sure that Koehler was right. The case against a Lysimache II therefore disappears.

It will be seen that, by positing that Lysistratos was a rather young *prytanis* in 341 and Polyeuktos an elderly archon of the Mesogeioi in 277/6, there are no difficulties in the *stemma*. I would further be inclined to identify the priestess of ii² 3455 (*c.* 300–290) with the priestess Lysimache referred to *c.* 270. There is no compulsion, but if Lysimache II is not the subject of 3455, she must have held office between aunt and niece, which is a little improbable.

(4) [. . . $^{c.8}$ . . .]τη Πολυεύκτου Βατῆθεν. Priestess in 255/4. Evidence under priestess (3). Koehler's restoration of Λυσιστρά]τη after her grandfather Lysistratos has been universally || accepted, perhaps rightly, but with a Phanostrate in the family as well, it would be foolish to pretend certainty.

(5) Καλλ[ι--], priestess in 220/19 (*IG* ii² 3461. 6, where ἐπὶ Καλλ[ι---ἱερείας] should be restored). This suggestion has not been made before, but it seems nearly certain. If any date is appropriate to an ἐρρηφορός, it is the dating by priestess of Athena Polias (cf. ii² 3470–3, 3486, 3497, 3515–16, 3554–5). The interpretation of the inscription has been held up by the desire to find in Καλλ[ι--] and Μενεκρ[άτης] two immediately following archons. Dow (*Hesperia* 2 (1933), 437 n. 3) pointed out there need only be one archon, and Kirchner suggested ἐπὶ Καλλ[ι- ἱερέως], which is very close to what seems to me right. The normal form later is, of course, ἐπὶ ἱερείας τῆς δεῖνα, but the fourth-century dedication ii² 4333 has [ἐπὶ---------]ς [ἱ]ερείας.

(6) Πεντετηρὶς Ἱεροκλέους Φλυέως (ii² 928, 3470–1) can only be dated by letter-forms and the little we know of the sculptors Kaikosthenes and Dies to the first half of the second century. It has been suggested in the main text that she descends from the marriage of Lysimache I into the deme of Phlya.

(7) Θεοδότη Πολυόκτου Ἀμφιτροπῆθεν. ii² 3472. Close to the preceding judging from letter-forms and the name of the sculptor, Kaikosthenes. The way in which she was an Eteoboutad can be determined. As we have seen, priestess (4) married Archestratos of Amphitrope. They had a grandson Polyeuktos (*PA* 11936). I do not see that Πολύοκτος can be anything but a vulgar spelling of Πολύευκτος, though *GAI*³ cites no parallel for the vowel change, and identification seems probable, with the dates, as far as we can determine them, fitting well.

(8) Ἀβρυλλὶς Μικίωνος Κηφισιέως. ii² 3477, and her gravestone with the insignia of the priestess, ii² 6398. Presumably daughter of Mikion (IV) (*PA* 10187), who was born a little before 200, since he appears in ii² 2314.44 (probably in 182) with

his grandfather still alive. She appears to belong to the third quarter of the century. A closer date for ii² 3477 can be obtained if we note [κ]ανηφορήσασαν [τ]ῶι Ἀπόλλωνι [τὴ]ν Πυθάϊδα. We know of no Delphic Pythais in the second century before that of the year of Timarchos, 138/7 (*FdD* iii. 2, *passim*). 138 is also a year of the great Panathenaia, and we note [κα]νηφορήσασαν [Π]αν[αθ]ήναια. 138 may actually be the date of the inscription. Koehler (*AM* 9 (1894), 301) thought that Habryllis proved the whole great family of Kephisia to be Eteoboutads, but they are more likely to have acquired Eteoboutad blood by marriage.

(9) Φιλωτέρα (Φιλτέρα) Παυσιμάχου. ii² 3473–4, 3870. We can hardly derive a clear *stemma* from the poem ii² 3474, but if she was the daughter of the Pausimachos of ii² 954, she cannot have been born much before 160. If she was in office late in the third quarter of the century, there is something wrong in the *stemma* of 3473 (Kirchner *ad loc.*), but hardly enough evidence to determine where the trouble lies.

(10) Χρυσὶς Νικήτου Περγασῆθεν. In office 106/5 (ii² 1136), and mentioned ii² 3484–6.

(11) Φιλίππη Μηδείου Πειραιέως. [Plut.] *X or.* 843B. Sister of the archon of 101/0, she presumably came to her office very shortly after Chrysis' only dated appearance. She is probably referred to shortly before (? 120–110) at Delos (*Inscr. de Délos*. 1869).

(12) Στρατόκλεια [-----] Φιλαίδου. ii² 3497. First century BC from letter-forms only.

(13) Μεγίστη Ἀσκληπιάδου Ἀλαιέως. ii² 3173. Only on the architrave of the Temple of Roma and Augustus, dated by the archonship of Areos, probably between 27 and 18 BC. Her probable grandfather's priesthood of Rome (ii² 2336.262) must now be dated 97/6 (Dow, *HSCP* 51 (1940), 111ff.). ||

(14) Ἀλεξάνδρα Λέοντος Χολλείδου. ii² 3155, 3516, 4341–3. Five inscriptions, but no possible lead to the date except the letter-forms. Probably early first century AD.

(15) Καλλιστ[-]. ii² 3515. Probably early first century AD.

(16) Ἰουνία Μεγίστη Ζήνωνος Σουνιέως. ii² 3283, 3535–7, 4175–6, 4242. Of these 4175–6 must belong to AD 44 or earlier, since P. Memmius Regulus is still *legatus Augusti*. 3535 is a little difficult. It is dated *c.* AD 57 by Kirchner, because Ti. Claudius Novius is hoplite-general for the fourth time and his eighth generalship is fixed to AD 61/2 by ii² 1990. This date is too late because 3273 also belongs to his fourth generalship and is still in the reign of Claudius. Greater precision is possible. The year is proved by 3535 to be a year of the great Panathenaia, and is therefore AD 47/8 or 51/2 (I follow West, *Corinth*, viii. 2, 34; the dates of Graindor (*Athènes de Tibère à Trajan*, 142 n. 1) seem faulty). Of these the latter is slightly preferable. It is nearly certain that 3270 and 4174 belong to Novius' first generalship, since the appearance of the number indicating repetition is so frequent in this period. They are already

Claudian, though 4174 (Regulus) shows that AD 44 is the latest date. Therefore, even allowing AD 40/1 as a possible though unlikely date for the first generalship, Novius would have to hold three generalships in seven years, with other repeating generals coming into question (Διονυσόδωρος Σο[---] Σουνιεύς has to have three before Claudius' death (3274), and Diokles of Hagnous reaches his fourth between AD 40 and 44 (4176)), and in fact we know of two other generalships which have to be fitted into these seven years (see ii² 3268, 4176). I therefore prefer AD 51/2 as the latest demonstrable date for Junia Megiste to be in office. There is one more inscription which provides indications of date, the dedication to Junia Lepida (4242). Of course, it could belong to any year up to 65, but the only plausible occasions for Junia Lepida to be in Athens are when her husband C. Cassius (*PIR*² C 501) was proconsul of Asia (40–1) or legate of Syria (44–51).

(17) Ἱπποσθενὶς Νικοκλέους Πειραιέως. ii² 4126–7, *Hesperia* 10 (1941), 238 no. 39. At this stage our apparently innocent investigation moves into an area more acutely disputed. Briefly, the position is this. 4126 is a bilingual dedication to L. Aquillius Florus Turcianus Gallus. He is now proconsul of Achaea, but he has previously been tribune of the Legion IX Macedonica, *Quaestor Imp. Caes. Aug.*, and proquaestor of Cyprus, apart from holding less controversial offices. The main difficulty is IX Macedonica, an unknown legion. When the inscription stood alone, there were no great problems. Unknown legions ought to be triumviral and of a date before 27 BC at the latest. *Imp. Caes. Aug.* is Augustus after 27 BC. The proconsulship of Achaea would be a little after 14 BC. Hipposthenis would find her place between priestesses (13) and (14). The difficulty is that we now have a duplicate dedication to Aquillius Florus from Corinth (*Corinth*, viii. 2, no. 54), and it is made by two *duumviri quinquennales*, a Ti. Claudius Anaxilas and another Ti. Claudius whose cognomen is lost. West, in republishing this inscription, drew attention to the fact that Ti. Claudius Anaxilas was known as an ordinary *duumvir* in Corinth towards the end of Claudius' reign, showed that the quinquennial duumvirates probably fell in AD 52, 57, 62, 67, and concluded that AD 52 is the most likely date for Anaxilas' quinquennial duumvirate. On this view IX Macedonica is simply a mistake or an unknown title of IX Hispana. The reference to *Imp. Caes. Aug.* is to Gaius. It is a possible title for him (cf. *IG* vii 2711.21), and Florus might wish to conceal his connections with him, yet boast his services as quaestor of the *princeps*. Lastly, Florus' multiple name is more suitable to the Claudian than to the Augustan period. Groag at first accepted this case (*PIR*² A 993), but later recanted (*Reichsbeamten von Achaia* 15–16). He maintained that the Ti. Claudius Anaxilas mentioned here must be a grandfather of the Claudian *duumvir*, and was fortified in returning to the original arguments by the fact that the || Corinthian copy adds to the proquaestorship of Cyprus '*ex auctoritate Augusti*', which suggests a reference to the extraordinary

commission of P. Paquius Scaeva in Cyprus early in Augustus' reign (*ILS* 915). I prefer West's date. The Corinth dedication is by two Tiberii Claudii, and in the absence of evidence of an occasion for grants of citizenship in Corinth by Tiberius or his father, a Claudian date is more likely. It will be seen that the Athenian evidence is indecisive. When the long-delayed study of fashions in imperial letter-forms is made, it may clinch the matter. If it were possible to determine the name of the priestess of ii² 3277, the answer might emerge, but I suspect that this is now impossible. Further discussions of this problem by Von Premerstein (*Abh. München* xv (1934), 217) and Oliver, *AJP* 69 (1948), 435 add little.

POSTSCRIPT. I leave the case for a Claudian date as it went to the printer, but its difficulties are increasing. Professor Syme draws my attention to Suetonius, *Tib.* 7. 2, which is evidence for a Claudian *clientela* in Greece in the first century BC and offers a possibility that there might be two Tiberii Claudii in Corinth under Augustus. More serious doubts are raised by a forthcoming revision of the Corinthian coinage by Mr D. M. MacDowall, who has generously discussed these problems with me. He will show that the coins of Anaxilas as ordinary *duumvir* fall at the end of Nero's reign in 65 or 66 and not at the end of Claudius'. If this is the same Anaxilas, we will have to abandon one of West's postulates, that an ordinary duumvirate ought to fall before a quinquennial one, for we will have Anaxilas as *quinquennalis* thirteen or fourteen years before his ordinary duumvirate. (West's arguments against a Neronian date for the inscription remain sound.) This is odd procedure, and the combined effect of these two points is to make me a good deal less certain of the Claudian date of the Corinthian inscription.[37]

(18) Στρατόκλεια. ii² 3554. First century AD, from letter-forms only.

(19) Νη[--]. ii² 3543. Priestess in the archonship of L. Flavius Flamma. The date, which is towards the end of the first century AD, is discussed by Notopoulos, *Hesperia* 18 (1949), 26.

(20) [---] Μηδείου. ii² 4247. I accept Kirchner's date at the end of the first century AD, without great conviction. There seems no adequate reason for restoring Λαδάμηα.

(21) Φλαβία Φαιναρέτη. ii² 3582–3, 4061 (?), 4210, 4345. If [ἐξακοσ]ίων is the right restoration in 3582, as considerations of space certainly seem to indicate, her term of office extends over the year of the reduction of the numbers of the *boule* from 600 to 500, since she appears with the Council of 500 in 4210. This does not

[37] [MacDowall read a paper on the Corinthian coinage to the Royal Numismatic Society in November 1965 (see *NC* 7th ser. 6 (1966), xxiv) but did not publish it. Most recently Amandry, *Le Monnayage des duovirs corinthiens* 19–22, 24–6, has dated Anaxilas' ordinary duumvirate AD 67/8. On Aquillius Florus, Thomasson, *Laterculi Praesidum* i. 189–90, cites nothing more recent than Groag's *Reichsbeamten von Achaia*.]

help to date her very precisely, since it still seems doubtful whether AD 124/5 or 128/9 is the correct date for the change (see Graindor, *Athènes sous Hadrien*, 18ff. and Kirchner, *IG* ii² 3283, *Addenda*; Notopoulos (*TAPA* 77 (1946), 53–6) now argues for 127/8). The matter is complicated by the dispute over the position held by the dedicatee of 4210, (L.) Aemilius Juncus, *cos. suff.* AD 127. Was his position, attested also by *IG* v. i 485, where he is described as δικαιοδότης, praetorian or consular? Groag (*Reichsbeamten von Achaia* 64–5) plumps for an extraordinary office of *corrector* after his consulate. In any case, the inscription must be earlier than about AD 134, as we see immediately.

(22) Ἀθήνιον. ii² 2810, 3596, 5063a. Graindor (*Hérode Atticus* 28 n. 2, 34, 35) has shown that 3596 must be dated about AD 134 on our knowledge of the career of the father of Herodes Atticus. 2810 is perhaps rather later; but Professor Syme confirms my impression that its principal dedicator, L. Aemilius Carus, is not the consul of 143 or 144 (*PIR*² A 338; Syme, *JRS* 36 (1946), 167; Degrassi, *Fasti Consolari* 41), but a provincial. 'Consular families are unlikely to be flaunting Greek names so early.' ||

(23) Γ[-----]. ii² 3612. If the reading is right, it represents an unknown priestess, hardly much later than AD 150 from the *stemma* of the subject of the base.

(24) Σαβεινιανὴ Ἀμιλλώ. ii² 3678. Date quite indeterminate, but later than the preceding.

(25) Παυλλεῖνα Σκριβωνία Καπίτωνος. ii² 3199. Her father, Scribonius Capito, is archon in the inscription ii² 2247 + 2250 + 2484 (joined by Mitsos, Ἀρχ. Ἐφ. 1950/1, 48), at the end of the second century. ii² 2122 presumably belongs to the same year.

I add one doubtful priestess from the fourth century BC, [--]υλλα (ii² 4601), because she is in Toepffer's list. Koehler suggested [ἐπ]ὶ ἱερείας [Νικ]ύλλης, comparing ii² 1472. 9 (326/5 BC). This passage does not prove that Νίκυλλα Πρεσ[βίου] is priestess. It merely says that the priestess handed over to the *tamiai* a dedication by her. 4601 does not prove that [--]υλλα is a priestess of Athena Polias, though she may be.

Except for 5063a, where Athenion is specifically described as priestess of Athena, I have not used the evidence of the seats in the Theatre of Dionysus, because they prove nothing. However, it can hardly be a coincidence that so many of the priestess' names appear on them, 5104, 5105, 5107, possibly 5123 and 5159. I can think of several explanations, but none worth pursuing.

23

A note on IG i^2 *114*

The battered stone which bears the bouleutic oath and a decree of the demos about the boule has the same width as and is universally agreed to be a companion piece to *IG* i^2 115 = i^3 114. That text begins with a decree of 409/8 ordering the anagrapheis to obtain Draco's law on homicide (from the basileus, as R. S. Stroud will show[1]) and write it on the stone. On the face of it, they are merely to make a copy, and, though the stray voice has been raised to suggest that they altered the text, most discussion has been about the age of the text before them and the changes that it might have undergone before 409.

The bouleutic stone is unprotected by a decree, as it stands, and encouragement for those who might wish to suppose that it represents a revised text comes from Philochorus, 328F140, which suggests a change in legislation about the boule in 410/09. There has not, to my knowledge, been any very great enthusiasm for massive revision or new formulation. To put it at a minimum, τάδε ἔδοχσεν ἐλ Λυκ[ε]ίο[ι] (line 34)[2] and μὲ ἔναι θόαν ἐπιβαλῆν (line 41) do not sound like constitutional procedure or even formal language of 410, and there has been at least one attempt[3] to carry the whole document back beyond the Persian War. The archaisms convinced even Hignett[4] 'that it was to some extent a copy of an earlier law', but he found it 'incredible that in 410, when the full democracy had just been restored after an oligarchic interlude, a law on this subject should have been no more than a faithful copy of a previous law; some additional safeguards suggested by previous experience must have been inserted'.

Wade-Gery's text of line 43 runs *hόπος ἄν δόκει* [τõι] δέμοι τõ[ι

* Published in *JHS* 87 (1967), 132. The text of the inscription is re-edited by Lewis as *IG* i^3 105, retaining the line numbers cited here.

[1] [See Stroud, *Drakon's Law on Homicide*.]

[2] I quote by Wade-Gery's line numbers (*BSA* 33 (1932/3), 120–1).

[3] Cloché, *REG* 33 (1920), 28–35. [4] *History of the Athenian Constitution* 153f.

'A]θεναίον πλε[θύοντι], and readers have probably assumed, as I did myself, that the spaces represented by the first τõι were so worn or damaged as to be illegible.[5] This is however not the case, as may be seen quite clearly on *BSA* 33 (1932/3), pl. 15. The three spaces are in excellent condition. They were never inscribed with letters. Instead each space does have, rather left of centre, a vertical pair of points, similar to those used, in single pairs, at least in lines 34, 44 and 50. The line runs *h*όπος ἂν δόκει : : : δέμοι τõ[ι 'A]θεναίον πλε[θύοντι].

Even if one were to assume that a major break in the structure of the text at this point for no very obvious reason occasioned a triple punctuation instead of a single one, the thought of a clause ending *h*όπος ἂν δόκει and another beginning δέμοι τõ[ι 'A]θεναίον πλε[θύοντι] is not attractive. The traditional restoration must be correct, and the [τõι] once appeared somewhere. Where the somewhere was, I do not know, but it was not on this stone, which was carved by a careful man transcribing a damaged original with such fidelity that he preferred to mark three blank spaces which he could not read rather than make what appears to us the easiest of conjectures. This stone was put up on the Acropolis and I imagine the original had been there too, just as the Draco code came from the basileus and went back on stone to the Stoa Basileia.

[5] Velsen and Koehler seem to have reported part of a crossbar, whence τ[õι] in first and second editions of *IG*.

24

The epigraphical evidence for the end of the Thirty

There is a good deal of epigraphic evidence relevant to the end of the Thirty and to the restoration period which followed, but there is no single place in which it has been collected. I do not propose to discuss it all; some of the problems are very complex. That is the case, above all, with the documents relevant to the nomothetic process which started after the fall of the Four Hundred and rumbled on until the prosecution of the scribe Nikomachos; discussion of the fragments of the 'walls' and possible erasures on them is likely to be indefinitely prolonged.[1] The notorious text, *IG* ii^2 10, concerning rewards for non-citizens, has, in my time, acquired the new fragments identified by Daphne Hereward.[2] It has had a good deal of attention, and there is some measure of agreement about the size of the name-lists,[3] though not about much else; what we perhaps needed even more was new fragments of the decree itself, without which there can be no certainty about the date or nature of the awards. The parallel text, the honours for citizens, the Heroes of Phyle, got off to a sensational start with its identification by || Raubitschek,[4] but, perhaps rather surprisingly, no new fragments have been identified since, despite careful study of all fragmentary name-lists by Don Bradeen. Those who have studied the 390s will testify how important it would be to have a complete list of those with that particular claim to the *eunoia* of the *demos*. The decree of Theozotides about orphans which Stroud

* Published in Piérart (ed.), *Aristote et Athènes* (1993), 223–9.

[1] See e.g. Dow, *Proc. Mass. Hist. Soc.* 71 (1953–7), 3–24, *Hesperia* 30 (1961), 58–73; Fingarette, *Hesperia* 40 (1971), 330–5. For problems about the exhibition of these fragments see Ostwald, *From Popular Sovereignty to the Sovereignty of Law* 519.

[2] *BSA* 47 (1952), 102–17 (*SEG* xii 84).

[3] Krentz, *Phoenix* 34 (1980), 298–306; Osborne, *Naturalization in Athens* D6; Whitehead, *LCM* 9 (1984), 8–10.

[4] *Hesperia* 10 (1941), 284–95 (*SEG* xxviii 45); see also Hansen, *Carmina Epigraphica Graeca* 431.

published in 1971[5] has at least added a little knowledge about the official language of the post-restoration period; it might have added a great deal more, had its surface not been destroyed by the acids of the Great Drain; perhaps it should be considered whether it might still add more if subjected to laser-beam photography from the back; the same, incidentally, might be said even more strongly about the naval law among the nomothetic texts (*IG* i^3 236),where we can actually count the lines without being able to read them. More recently, Michael Walbank[6] has identified substantial fragments of the accounts of the confiscated property of the Thirty, a not insignificant text, which can also be linked to the history of Athena's treasures.[7] The documents of the Treasurers of Athena and the Other Gods have also made their own contribution, with clearer guidance about the reabsorption of Eleusis into the Athenian state and the transfer of some Eleusinian treasures into their care.[8]

I discuss here in more detail a pair of documents concerning these Treasurers whose evidence for the end of the oligarchic régime seems to me to have been unduly neglected in political treatments. The first is an inventory. Two fragments, *IG* ii^2 1370 and 1371, were edited separately by Kirchner in 1927. A. M. Woodward drew Kirchner's attention to a substantial improvement which could be made by joining them; the heading of a text of 403/2 now emerged, and Kirchner published this || revised text in 1931 (*IG* ii^2. 2. ii supp., p. 797). Woodward (adding the name of A. B. West *honoris causa*) published a fuller treatment (adding two further fragments, *IG* ii^2 1384 and 1503) in *JHS* 58 (1938), 78–83; there had been some discussion by W. S. Ferguson, *The Treasurers of Athena* 57 n. 1, 150. He now revised the text, for the worse, as was later shown by Tréheux.[9] I give Tréheux's text:

[5] *Hesperia* 40 (1971), 280–301 (*SEG* xxviii 46; see also *SEG* xxxiii 67). The date has been challenged by I. Calabi Limentani, *Studi in onore di Cesare Sanfilippo* vi. 116–28 (see (*SEG* xxxvii 65)).

[6] *Hesperia* 51 (1982), 74–98 (*SEG* xxxii 161).

[7] It is reasonably certain that the processional vessels made from the confiscated property of the Thirty were recorded on a special stele in 402/1 (*IG* ii^2 1372 + 1402 + Ἀρχ. Ἐφ. 1953/4, β (1958), 107–12 (*SEG* xvii 39; see also Tréheux, *EAC* 3 (1965), 44–62 (*SEG* xxiii 82)); see Walbank, *Hesperia* 51 (1982), 97–8 (with a slight confusion).

[8] Woodward, *JHS* 58 (1938), 70; Stroud, *Hesperia* 41 (1972), 422–3; Lewis and Woodward, *BSA* 70 (1975), 183–9 (with important modifications collected in *SEG* xxx 101); Clinton, *Studies Presented to Sterling Dow* 51–60 (*SEG* xxxiv 114–16), about which I have several reservations.

[9] *EAC* 3 (1965), 41–4, 62–7 (*SEG* xxiii 81).

[θ ε] ο ί
[τάδε οἱ ταμίαι τῶ]ν ἱερῶν χρημάτων τῆς Ἀθηναίας
[καὶ τῶν ἄλλων θεῶ]ν οἱ ἐπὶ Εὐκλείδο ἄρχοντος Λυσικ-
. . . . . [c. 14] . . . . . , Θεογένης Θριάσιος, Ἀντιφάνης
[. . . [c. 8] . . . οἷς Μισγο]λας Κολλυτεὺς ἐγραμμάτε-
[υε, . . . [c. 7] . . . παραδεξάμενοι παρὰ] τῶν προτέρων τ[αμ]-
[ιῶν τῶν ἐπὶ Πυθοδώρο ἄρχοντος, οἷς Δ]ρομοκλε . . . .
[. . . [c. 8] . . . ἐγραμμάτευε, παρέ]δοσαν [ταμίαις τοῖς ἐπὶ]
[Μίκωνος ἄρχοντος . . [c. 6] . . Τει]θρασίωι, ... | . . . [c. 9] . . .
[. . . . . [c. 23] . . . . . κ]λεῖ Αἰξωνεῖ, Ῥίν[ωνι . . .]
[. . . . . [c. 21] . . . . . , ο]ἷς Κλείσοφος Εὐων[υμεὺς]
[ἐγραμμάτευεν, ἐν τῶι νεῶι] τῶι Ἑκατομπέδωι· *vacat*

Lines 6–7: One of Woodward's motives for revising the text in 1938 was embarrassment about the gap of seven or eight letters which his earlier text required before παραδεξάμενοι. Ferguson had tried to fill this gap with ἄγραφα, assuming that the *tamiai* of the previous year had made no lists. Tréheux discusses the implications at length and sees no alternative, but it remains a problem. Ferguson also preferred τῶν ἐν Ἀναρχίαι ἀρξάντων, which Tréheux doubts; there can be no certainty, on the basis of this text, whether the previous *tamiai* were identified as those of Pythodoros's archonship or as those of the 'anarchy'.

Line 7: it is not obvious why Tréheux abstains from the obvious Δρομοκλε[ίδης].

Lines 8–11: it results from Tréheux's return to Woodward's original restorations that there should be six *tamiai* named for 402/1. In 1938 Woodward had opted for seven.[10]

Lines 10–11: Tréheux also abstains from Ῥίν[ωνι Παι|ανιεῖ. The restoration goes back to Koehler. Rhinon is known from *Ath. Pol.* 38.3–4 as a member of the Ten who was at once elected *strategos* under the || democracy. Develin, *Athenian Officials 684–321 B.C.* 201 is also 'not confident of the identification', but the name is far from common.

It is certain that the board of 404/3 was mentioned in some form, and almost inevitable that they were mentioned in the language of a regular

[10] In *JHS* 58 (1938), 84–5 he thought that there was supporting evidence for seven in *IG* ii² 1372 + 1402. He later (Ἀρχ. Ἐφ. 1953/4, β (1958), 111) argued that the board of 402/1 was not listed there at all; Tréheux (*EAC* 3 (1965), 61) thinks that it was and that it did number six.

paradosis. I think that this in itself probably proves that they stood *euthunai*, and there is further evidence. It had long been observed that, in a Lycurgan list of bronze stelai dedicated by *tamiai*, it was possible to restore [στήλη ταμ|ιῶν τῆ]ς θεο[ῦ κ]αὶ τῶν ἄλλων [θεῶν ἐπὶ Πυθοδώρου ἄρχ|οντος] (*IG* ii² 1498. 21); there are other archons with nine letters in the genitive during the period of the combined board, but all the surviving names in this text are from the fifth century. More striking is the second of my documents, an expense-account, *IG* i³ 380, first published in 1963. Though in Ionic letters, it appears to be fifth-century; that it comes from an oligarchic régime is suggested by the appearance of the phrase κα]τὰ ψήφισμα βολῆς. Fragmentary and difficult though it is, it is hard to see what else it can be but the accounts of the *tamiai* of 404/3. If this is so, they certainly stood their *euthunai* on them and had them published, and that is the conclusion which Peter Krentz also comes to in his further investigations of the document.[11] We therefore have some *prima facie* evidence for continuity in at least one branch of the administration through the period of the Thirty.[12]

We now have to establish the nature of the transfer-process and when it happened. For the *tamiai*, this would involve several elements.

(1) Since they had accounts to pass, the *logistai* were involved. Despite some doubts expressed about this for the fifth century,[13] I see no essential difference between the fifth-century system and that obtaining at the time of the *Ath. Pol.* (54.2) beyond a change in numbers from thirty[14] to ten (plus ten *synegoroi*). However, we cannot be sure that there were *logistai* under the Thirty nor can we know how soon the democracy created such a board.[15] ||

(2) It is a more important question to determine where the *tamiai* of

[11] *Hesperia* 48 (1979), 54–63 (= *SEG* xxix 19) at 63. It would follow, as Tréheux saw, that Ferguson's argument to support ἄγραφα in line 6 of our first text, that no documents of the Thirty would have survived, is flawed.

[12] I should note that the case presented by Ferguson, *The Treasurers of Athena* 8–15, for supposing that a reverse secretary-cycle operated for the secretaries of the *tamiai* continuously from 411 to 385, is not currently accepted.

[13] Rhodes, *The Athenian Boule* III, *Comm.* 598. Piérart, *AC* 40 (1971), 526–73 at 564–8, has a fuller treatment, with which I concur.

[14] For avoidance of boards of thirty after 404 cf. *Ath. Pol.* 53.1.

[15] Rhodes, *The Athenian Boule* III, *Comm.* 560, rightly observes that Lys. xxx.5 implies that an operation every prytany by a board of *logistai* appointed from the *boule*, as described in *Ath. Pol.* 48.3, was of very long standing, so that the restored democracy will have needed *logistai* of this type at least as soon as its magistrates started to operate.

404/3 stood their *euthunai*. Our only explicit statement about *euthunai* for 404/3 (*Ath. Pol.* 39.6) strictly, it would appear, applies only to the Thirty, the Ten, the Eleven and the Ten in the Piraeus; those who had held office in the Piraeus would stand them ἐν τοῖς ἐν Πειραιεῖ, those who had operated in the city would be given the concession of a court limited to property-holders (ἐν τοῖς τὰ τιμήματα παρεχομένοις). This ought to have applied to 'the second Ten', who λαβόντες τὴν ἐπιμέλειαν ἐν ὀλιγαρχίᾳ τὰς εὐθύνας ἔδοσαν ἐν δημοκρατίᾳ, and were not objected to by anyone from City or Piraeus (*Ath. Pol.* 38.4). These are extraordinary officials (or, in the case of the Eleven, had behaved in extraordinary ways), who might be thought to need special provision, but my feeling is that the restriction on the class of those conducting the *euthunai* might well also have applied to the *tamiai*, if their *euthunai* were still outstanding; there would be no reason to treat them more severely than the officials who had formed the core of the oligarchy. So far, however, it remains a possibility that they had stood their *euthunai* under whatever arrangements the oligarchy possessed.

(3) To these two elements common to those magistrates who handle public money, the *tamiai* (and no doubt other officials with temple-objects in their care) add a third, the *paradosis*. Every surviving prescript of a treasure-inventory records a transfer between two boards.[16] It is seldom described in detail, and it may not always be recognised what is involved in the bland sentence in *Ath. Pol.* 47 παραλαμβάνουσι δὲ τό τε ἄγαλμα τῆς Ἀθηνᾶς καὶ τὰς Νίκας καὶ τὸν ἄλλον κοσμὸν καὶ τὰ χρήματα ἐναντίον τῆς βουλῆς. We do have a description (*IG* i^3 52.A18–21) of how the first Treasurers of the Other Gods were to receive the objects of which they were going to be in charge from their existing holders, weighing them and counting them in front of the *boule*; at the same time, it appears (*ibid.* B26–9), steps were taken to improve the record of the weights and numbers of Athena's treasures. Though the prescripts seldom thought fit to record it, we can hardly doubt that the formula which, for a brief period in the early fourth century,[17] adds || ἀριθμῶι καὶ σταθμῶι, 'by number and by weight', to the description of a *paradosis*, always reflects the correct procedure. What was transferred had to be weighed and counted, and the presence of both boards and the *boule* is

[16] Though our text in fact refers to three boards and two transfers, not all boards find it necessary.

[17] *IG* ii^2 1377.9 (400/399), [1378.12] (399/8), 1388.16 (398/7). Koehler had once restored it in our text (see *IG* ii^2 1371.5), but there is no room for it.

presumed. It is not easy to compute how many objects were involved in 403, but it will not have been a short operation if it were taken seriously. By 384, at the latest, the gold and ivory statue also had to be checked against the details on a bronze stele,[18] and a check of some kind surely goes back to its original dedication (cf. Plut. *Per.* 31.3).

This *paradosis* should normally have taken place at the time of the Panathenaea, Hekatombaion 28 (*IG* i³ 52.A27–9).[19] Whatever one thinks about the chronology of the Thirty,[20] it is not easy to think that conditions were favourable in the city at that date[21] for the procedures involved.[22] It would seem much more likely that the *paradosis* was deferred until well after the Panathenaea, and that the term of the *tamiai* of 404/3 was prolonged until conditions were more favourable for relieving them of their responsibilities. In fact, we may note, the responsibilities of the *tamiai* had been increased; we find that they are paying for inscribing decrees in 403/2,[23] and this probably means that they were entrusted with other responsibilities as well.[24]

One consideration may give us pause. Why are only three *tamiai* listed for 403/2? Other short boards of *tamiai* are known, but not this || short.[25] If

[18] *IG* ii² 1407.6–7, restored, more simply, as [τὸ ἄγαλμα ἐντελὲς κατὰ τὴν στήλην τὴν] | χαλκῆν τὴν ἐν τῶι Παρθενῶνι [ὁμ]ολογόμενον by Woodward, *HSCP* Supp. 1 (1940), 381, who further explores the next line, which has a reference to ivory and, probably, to checking weights against silver. I have argued that the procedure was annual in *Comptes et inventaires dans la cité grecque . . . en l'honneur de J. Tréheux* 300–1.

[19] This text is sufficient for the fifth century, I think, and should certainly apply to 403. There is in fact no clear evidence for the term of office of the *tamiai* in the fourth century; although I remain convinced that *Ath. Pol.* 43.1 refers to the financial year and not to a Panathenaic quadrennium, their absence from it is not evidence.

[20] See Krentz, *The Thirty at Athens* 147–52, for an introduction to the problems.

[21] Extrapolation from Meritt's table of Julian dates (*Proc. Amer. Philosoph. Soc.* 115 (1971), 114) would produce something like July 25th, but I would not insist on this.

[22] It cannot, of course, be argued from our text, drawn up in summer 402, that the board of 403/2 had been called the ταμίαι ἐπὶ Εὐκλείδου ἄρχοντος right from the beginning of their tenure.

[23] Henry, *Chiron* 12 (1982), 103–4.

[24] See Krentz, *Hesperia* 48 (1979), 60, denying Woodward's suggestion that the *hellenotamiai* may have survived into 404/3. Their appearance in *SEG* xxviii 46 (see note 5) complicates the matter.

[25] Since we cannot establish the line-length of *IG* i³ 380, it remains uncertain how many *tamiai* there had been in 404/3. Woodward (*Hesperia* 32 (1963), 148) seems to have thought that there were seven plus a secretary; Krentz arrives at a full board of ten, taking it in turns to act as secretary (this would be unparalleled). For 402/1 there were

the paradosis was deferred, the explanation is probably that it was not possible to find more than three *pentakosiomedimnoi* to serve under the restoration. It might further follow from that that the laxity in the selection of *tamiai* attested in *Ath. Pol.* 47.1 had not yet started; 403/2, indeed, is not a year in which we expect the prerogatives of the rich to be infringed. However, we cannot totally exclude the possibility that there had been a regular *paradosis* at the proper time before the restoration, and that there had originally been a larger board of whom only three *tamiai* were still in Athens at the end of their year in 403/2. The rest can have gone to the new state of Eleusis or elsewhere. If that is the correct reading of the situation, the restoration would, again, be being tolerant of the proprieties. The Five Thousand, by contrast, had replaced the *tamiai* of the Four Hundred *en bloc* (*IG* i^3 313, 337).

An exploration of this kind, searching epigraphic administrative documents for political implications, is perhaps very modern in its approach, and it surely would not have occurred to anyone in the fourth century BC to do anything of the kind. This is not to say that the authors and sources of the *Ath. Pol.* were unaware of epigraphic evidence.[26] The epigram in 7.4 comes from someone's direct observation, and what is said about the old-style secretaries in 54.3 implies at least a willingness to read the larger letters on epigraphic texts. But none of the texts which I began by listing make any detailed appearance in ancient literary texts, except for the epigram on the Heroes of Phyle. Many opportunities were thus overlooked. Even if the texts had been searched, we may doubt whether they would have given rise to 'lateral thinking', to an indirect approach to specific problems of the type which I have been conducting. The nearest we come to that remains the argument by some nameless hero about the absence of the Areopagus from Draco's laws (Plut. *Sol.* 19), but it may be dangerous to refer to that.

probably six (see above), in 401/0 probably seven (*IG* ii^2 1372 with *SEG* xxiii 82), in 400/399 a full board of ten (*IG* ii^2 1374, 1375), in 399/8 nine (*IG* ii^2 1375, 1377, 1378), in 398/7 ten (*IG* ii^2 1388, 1391, 1392). Either it became easier to recruit *pentakosiomedimnoi* or the rules were being relaxed.

[26] Stroud's paper in Piérart (ed.), *Aristote et Athènes* 203–21, is also relevant here.

25

On the financial offices of Eubulus and Lycurgus

I EUBULUS

My main concern here is with Lycurgus, but it is a necessary preliminary to say something about Eubulus, the more necessary because, although the correct view of the position of οἱ ἐπὶ τὸ θεωρικόν under Eubulus has been clearly stated by Lipsius in 1905, Ferguson in 1911 and Busolt–Swoboda in 1926,[1] general histories (most recently, Hammond) persist in saying that οἱ ἐπὶ τὸ θεωρικόν had a four-year term.[2] It will therefore be as well to put out the evidence yet again, although my discussion of this point contains no novelties.

Ath. Pol. 43.1 says: τὰς δ' ἀρχὰς τὰς περὶ τὴν ἐγκύκλιον διοίκησιν ἁπάσας ποιοῦσι κληρωτάς, πλὴν ταμίου στρατιωτικῶν καὶ τῶν ἐπὶ τὸ θεωρικὸν καὶ τοῦ τῶν κρηνῶν ἐπιμελητοῦ· ταύτας δὲ χειροτονοῦσιν, καὶ οἱ χειροτονηθέντες ἄρχουσιν ἐκ Παναθηναίων εἰς Παναθήναια. Two interpretations are possible, that these officials have a four-year term, from Great Panathenaea to Great Panathenaea, or that they have an annual term defined by the date of the Panathenaea, Great or Small, rather than that of the civil year, that is, that they enter and demit office on Hekatombaion 28.

* This paper was written *c.* 1957 and circulated in Oxford in the late 1950s and early 1960s: see G. L. Cawkwell, *JHS* 83 (1963), 47 unnumbered note (general indebtedness), 58 n. 68 (Hyp. *Dem.* 28), 55 n. 2, *CQ* n.s. 13 (1963), 135–6, 19 (1969), 169 n. 2 (law of Hegemon); Davies, *A.P.F.* 351 (Lycurgus' twelve years beginning 336); Rhodes, *The Athenian Boule* 105 n. 2 (general indebtedness), 106 n. 4 (Hyp. *Dem.* 28), 235 and *Commentary on the . . . Ath. Pol.* 515 (law of Hegemon), 239–40 (abolition of ἐπιμεληταὶ τῶν νεωρίων). In 1993 Lewis revised sections I–II and added section III.

[1] Lipsius, *Das attische Recht* 92–3 n. 152; Ferguson, *Hellenistic Athens* 474–5; Busolt ed. Swoboda, *Griechische Staatskunde* ii. 1055 n. 3.

[2] *HG* 531. The four-year term has been explicitly defended by R. Develin, *ZPE* 57 (1984), 133–8.

Three boards of officials in the fifth century can be shown to have adopted the second course. The Treasurers of Athena, the Treasurers of the Other Gods and the Hellenotamiae were all annual boards, who ἐδίδοσαν τὸν λόγον ἐκ Παναθηναίων εἰς Παναθήναια.[3] It is true that the four boards of Treasurers of Athena, who served during a Panathenaic *penteteris*, combined as the Four *Archai* to publish their accounts (though the Treasurers of the Other Gods did not, since *IG* i³ 383 begins with the board of 429/8), but it is clear from these accounts that each annual board completed its *paradosis* at the end of its financial year and from ML 58 = *IG* i³ 52.A24–7, that they stood their *euthyna* then. On the fifth-century evidence, we should not suppose that ἐκ Παναναθηναίων εἰς Παναθήναια means 'from Great Panathenaea to Great Panathenaea'. If Aristotle meant this of the Fourth century, he could have said so, and in fact he does in effect say so of the *athlothetai* (60.1): οὗτοι δὲ δοκιμασθέντες ἄρχουοι τέτταρα ἔτη.

It therefore seems much more likely that the second view is to be preferred. Let us consider the evidence as it bears on Aristotle's three offices.

(1) The ταμίας στρατιωτικῶν. The three named ταμίαι στρατιωτικῶν[4] who antedate the Lamian War give us no sequence, but none shows any signs of operating for more than one year. One indeed, Kallias, is described as τοῦ ταμιεύσαντος στρατιωτικῶν ἐπὶ Χαιρώνδου ἄρχοντος (338/7), which would be odd language, if he had in fact served a four-year term from 338 to 334. After the liberation of Athens in 307, the position is clearer, though it may not be relevant, since the change of office now seems to come before Hekatombaion 28, and where there is one change there may be two. The ταμίας στρατιωτικῶν for 306/5 is Ἄβρων Βουτάδης (*IG* ii² 1491.123), that for 305/4 is Φίλιππος Ἀχαρνεύς (*IG* ii² 1491. 130, 136, 138).

(2) οἱ ἐπὶ τὸ θεωρικόν. There is no evidence to suggest that Eubulus, Aphobetos or Kephisophon served a four-year term. Boeckh argued

[3] *IG* i³ 292 etc.; ML 58 = *IG* i³ 52.A27–9; for the Hellenotamiae cf. Meritt, *Athenian Financial Documents* 126, which still seems to hold despite later dispute.

[4] Νικήρατος Νικίου Κυδαντίδης, 344/3: *IG* ii² 1443.13–14. Καλλίας Ἄβρωνος Βατῆθεν, 338/7: [Plut.] *X Or.* 832F–833A: discussed in text. Δημάδης Δημέου Παιανιεύς, 334/3: *SEG* xxi 552 (*IG* ii² 1493).11–13 etc. Develin claims Plut. *Praec. Ger. Reip.* 818E as evidence for Demades as treasurer in 332/1 and thinks that at any rate by this time the office was quadrennial (*ZPE* 57 (1984), 133–8 at 135, cf. *Athenian Officials* 380, 387; earlier, F. W. Mitchel, *TAPA* 93 (1962), 213–29 at 219–25).

from Aesch. III.24 that the fact that the archon in whose year Demosthenes was elected to the position could be named proved a one-year term, but this does not seem cogent.[5] On the one-year view his term was 337/6, on the four-year 338–334. The second view clashes with Lycurgus' tenure of a different office, and has for this reason, I suppose, been generally unpopular even among those who hold to a four-year term for Eubulus.

(3) ὁ τῶν κρηνῶν ἐπιμελητής. It cannot be proved that *IG* ii² 215 of 346/5 refers to this office, and not to some more temporary *epimeleia*, but it is the only permanent office which can be restored in line 12, and the phraseology is closely parallel to that of ii² 338 which does so refer. If the restoration of 215 is correct, the situation is clear. The honorand was elected ἐπ[ὶ] Θ[εμιοτοκλέους ἄρχον]τος (347/6), and is being honoured in 346. Only a one-year term meets the facts. In 338 the position is clear. The honours are given to Pytheas, eleven days after the Panathenaea of 333, and will be conferred ἐπειδὰν τὰς εὐθύνας δῶι. On the four-year view, either he has served 338–334, and his *euthyna* has been deferred over a year, despite his obligation to present his accounts within thirty days (Harp. *s.v.* λογισταὶ καὶ λογιστήρια: 194.7 Dindorf = λ 24 Keaney), or his honours are being conferred on him three years before the end of his term, 334–330. It is clearly much more likely that he is being honoured eleven days after the end of a one-year term 334/3.

There is thus no evidence of any kind to suggest that οἱ ἐπὶ τὸ θεωρικόν ever served a four-year term. Any statement that Eubulus assumed control of Athenian finance as ἐπὶ τὸ θεωρικὸν at the Great Panathenaea of 354 or that he served a four-year term from 354 to 350 lacks all ancient authority. We cannot even define the year in which a special board was created for the *theorika*. The stratiotic fund certainly existed by spring 346,[6] but payments

[5] *Staatshaushaltung*³ i. 224–5 with 225 n. *a*.

[6] G. Glotz, *RH* 170 (1932), 385–97, argued from Dem. I.19 with III.10–11 and *IG* ii² 207.*bcd*11 that the stratiotic fund was created by Demosthenes in 349/8; G. L. Cawkwell, *Mnem.* 4th ser. 15 (1962), 377–83, exposed the weakness of Glotz's argument and suggested that the fund might have been created as early as the 390s; Rhodes, *The Athenian Boule* 105 with n. 3, saw allusions to the fund in [Dem.] XLIX.12, 16 (373), L.10 (362).

M. J. Osborne separated *IG* ii² 207.*a* from *bcd*, and eventually dated both parts in 364/3 (*BSA* 66 (1971), 297–321; *Naturalization* D12: i.52–4, ii.61–80). In *a* Pittakys read

from it were then being made by the *apodektai* (*IG* ii² 212.42–4), and the first known ταμίας στρατιωτικῶν dates from 344/3 (*IG* ii² 1443.13). A similar time-lag may have existed with the *theorika* even after Eubulus assumed control of finance, whatever that may be held to mean. However, to exclude a four-year term does not also exclude the possibility that the office could have been held more than once or in successive years. Motzki's attempt to shake the authority of the *Ath. Pol.* on this and on other matters[7] is not very convincing, but there may be a seed of truth in it. But there is no need to argue as he does[8] that Eubulus' whole activity proves that he was ἐπὶ τὸ θεωρικόν until his fall, and that therefore Aristotle is wrong about iteration of magistracies. Aristotle is explicit in 43.1 in including οἱ ἐπὶ τὸ θεωρικόν as an *arche*, and it should therefore come under the provisions of 62.3 forbidding iteration except for τὰς . . . κατὰ πόλεμον ἀρχάς, though it might be possible to hold that this is true of the 320s and not of the 340s. Iteration is certainly not a necessary interpretation of Aesch. III.25: διὰ δὲ τὴν πρὸς Εὔβουλον γενομένην πίστιν ὑμῖν οἱ ἐπὶ τὸ θεωρικὸν κεχειροτονημένοι ἦρχον μέν, πρὶν ἢ τὸν Ἡγήμονος νόμον γενέσθαι, τὴν τοῦ ἀντιγραφέως ἀρχήν, ἦρχον δὲ τὴν τῶν ἀποδεκτῶν καὶ νεωρίων ἀρχήν, καὶ σκευοθήκην ᾠκοδόμουν, ἦσαν δὲ καὶ ὁδοποιοί, καὶ σχεδὸν τὴν ὅλην διοίκησιν εἶχον τῆς πόλεως. This may mean that Eubulus was always a member of the board, but it may equally mean that he was always known to be behind the board.

In any case, this passage would seem to exclude Motzki's view that Eubulus assumed in 350 a position analogous to that of Lycurgus, an office with the later technical name of ὁ ἐπὶ τῇ διοικήσει. Aeschines' language leaves no room for such a superior official, nor is such a specific function to be found in e.g. Dein. 1.96, Plut. *Praec. Ger. Reip.* 812F, *Frat. Am.* 486D, *Phoc.* 7. The only reference from the 340s to anybody specifically charged with the *dioikesis* is Aesch. II.149: Aeschines' brother Aphobetos καλῶς δὲ καὶ δικαίως τῶν ὑμετέρων προσόδων ἐπμεληθείς, ὅτε αὐτὸν ἐπὶ τὴν κοινὴν διοίκησιν εἵλεσθε. This post appears to rank between an embassy to Persia and having legitimate children as a claim to fame. It does not seem easy to combine it with sole control of Athenian finance, however nominal, from 350 to 346,

Nikomachos, the archon of 341/0; Rangabé's Kallimachos (349/8) was defended by R. A. Moysey, *ZPE* 69 (1987), 93–100; 341/0 by D. H. Kelly, *ZPE* 83 (1990), 96–109; see also R. Develin, *ZPE* 73 (1988), 75–81. Pittakys' reading may not have been correct, but Rhodes regards a date in the 340s as much more likely than one in the 360s.

[7] *Eubulos von Probalinthos* 21–4, 36–7. [8] *Ibid.* 37–8.

and the most obvious solution is that Aphobetos was ἐπὶ τὸ θεωρικὸν for one year. Eudoxos' functions (*IG* ii² 223.B10–12) seem to fall entirely within the *boule*.

Before we consider the implications of Aesch. III.25 for the post-Eubulan period, it may be helpful to collect the evidence for its meaning under Eubulus. The clear inference from it and the immediately preceding sentence (πρότερον μὲν τοίνυν, ὦ ἄνδρες Ἀθηναῖοι, ἀντιγραφεὺς ἦν χειροτονητὸς τῇ πόλει, ὃς καθ' ἑκάστην πρυτανείαν ἀπελογίζετο τὰς προσόδους τῷ δήμῳ) is that the office of *antigrapheus* was abolished during this period, and also that it had not been restored in 330. This second inference might be thought to be supported by the absence of this official from *Ath. Pol.* 54.3–5, and the apparent activity of a *demosios* in a similar capacity in 47.5. But the existence of the *antigrapheus* in 335/4 is guaranteed by his appearance in *IG* ii² 1700.217 = *Agora* XV 43.231.[9] Epigraphical evidence of his existence before Eubulus is confined to *IG* ii² 1740.55 = *Agora* XV 12.66, where [ἀντι]γραφεὺς seems a certain restoration.[10] For his absence under Eubulus, Raubitschek has drawn attention to *Hesperia* 3 (1934), 63 no. 54 = *Agora* XV 37, and has shown that there a ὑπηρέτης, a slave, is named after the γραμματεὺς τ[ῶι] δ[ήμωι καὶ τῆι] βουλῆι in the place occupied in *IG* ii² 1740 by the [ἀντι]γραφεύς. He therefore suggests that this *hyperetes* performed the functions performed earlier by the *antigrapheus*.[11] But Wilhelm, making independently the same restorations in *Hesperia* 3 no. 54, went on to find a reference to a *hyperetes* in *IG* ii² 2411.4 = *Agora* XV 72.5, at the end of the entry in one tribe in a *bouleutai* list.[12] This opens the possibility, or probability, that the responsibility of these *hyperetai* is merely of general assistance to the *bouleutai* of a particular tribe and need have nothing to do with financial operations for the *boule* as a whole.

The best evidence for the disappearance of the ἀντιγραφεύς under Eubulus comes from *IG* ii² 223.C = *Agora* XV 34.5, where he is not listed under the officials of the *boule*, but where we do find ἐπὶ τὸ θεωρικὸν Κηφισοφῶν. Kephisophon is clearly either a *boule* official replacing the *antigrapheus* or, much more likely, one member of οἱ ἐπὶ τὸ θεωρικόν detached for service

[9] A supposed appearance in 327/6 (Dow, *Prytaneis* 1.83) has been redated to 305/4 (*Agora* XV 58.80).

[10] Within the first half of the century the date is very uncertain: see Lewis, *BSA* 50 (1955), 19–20, and *Agora* XV *ad loc.*

[11] *Hesperia* 11 (1942), 305. [12] *Abh. Berlin* (1939) no. 20 = *Akademieschriften* iii.1–6.

with the *boule* in this capacity.[13] It is legitimate to say that the *antigrapheus* disappears.[14]

Aeschines further says that οἱ ἐπὶ τὸ θεωρικὸν ἦρχον τὴν τῶν ἀποδεκτῶν . . . ἀρχήν. The *apodektai* certainly existed in 347/6 (*IG* ii² 212.43), *c.* 335/4 (*Hesperia* 28 [1959], 239–47 = *Agora* xix L7, 19–20; *IG* ii² 222 = Osborne, *Naturalization*, D22), in 329/8 (*IG* ii² 1627.228), 327/6 (*IG* ii² 1628.630), and 326/5 (*IG* ii² 1628.427). I have found no case where one would expect their presence and does not find it. A gap in their activity is possible, but one can hardly say that the evidence is decisive one way or the other.[15]

ἦρχον δὲ τὴν . . . νεωρίων ἀρχήν. A board of νεωρίων ἐπιμεληταί is attested for 349/8 (*IG* ii² 1620.43, probably not more than seven names there), and we have one name for 348/7 (*IG* ii² 1622.549–51). The next directly attested board is that of 334/3 (*IG* ii² 1623.1–3) at the earliest. It may be held that there is a curious gap in our preserved records of the *epimeletai*. Of the records that we have from the 340s, two relatively small fragments in the normal style, *IG* ii² 1620 and 1621, are plausibly attributable to 349/8 or

[13] Rhodes, *The Athenian Boule* 235, used this text as evidence that before the law of Hegemon there was a single official ἐπὶ τὸ θεωρικόν, and he is still inclined to believe this (cf. *Commentary on the . . . Ath. Pol.* 515). He does not know whether he persuaded Lewis on this point or on the point argued in the next note. That there was always a board has been widely believed: e.g. Buchanan, *Theorika*, 57–60; Cawkwell, *JHS* 83 (1963), 47 with n. 4; G. E. M. de Ste Croix, *CR* n.s. 14 (1964), 191.

[14] Harp. *s.v.* ἀντιγραφεύς (39.5 Dindorf = α 153 Keaney) reads

> ὁ καθιστάμενος ἐπὶ τῶν καταβαλλόντων τινὰ τῇ πόλει χρήματα, ὥστε ἀντιγράφεσθαι ταῦτα· Δημοσθένης ἐν τῷ Κατ' Ἀνδροτίωνος (XXII.38; 70 = XXIV.178) καὶ Αἰσχίνης ἐν τῷ Κατὰ Κτησιφῶντος (III.25). διττοὶ δὲ ἦσαν ἀντιγραφεῖς, ὁ μὲν τῆς διοικήσεως, ὥς φησι Φιλόχορος (*FGrH* 328F198, known only from this reference), ὁ δὲ τῆς βουλῆς, ὡς Ἀριστοτέλης ἐν Ἀθηναίων Πολιτείᾳ (error from ἀντιγράφεται in 54.3–4?).

Other texts referring to two *antigrapheis* are Poll. VIII.98, Phot. *s.v.* ἀντιγραφεύς (α 2089 Theodoridis), *Lex. Seg.* 410.3, *Et. Sym. ap. Et. Mag.* 268H–I Gaisford; cf. for the first *Lex. Seg.* 197.24, for the second *Lex. Seg.* 185.14, Suid. *s.v.* γραμματεύς (γ 417 Adler).

Rhodes, *The Athenian Boule* 237–9, suggests that the *antigrapheus* of Demosthenes and Aeschines is the ἀντιγραφεὺς τῆς διοικήσεως of the lexicographers, while the *antigrapheus* of inscriptions later than 350 is the ἀντιγραφεὺς τῆς βουλῆς, and that the old *antigrapheus* was abolished when his duties were taken over by the theoric official(s) and either immediately or after a period in which there was no *antigrapheus* the title was given to one of the state's secretaries.

[15] Rhodes, *The Athenian Boule* 239, suggests that the *apodektai* may have been subordinated to the theoric official(s) without actually being abolished.

348/7,[16] and only one inscription, *IG* ii² 1622, remains to fill the gap for 347/6 to 335/4 inclusive.

IG ii² 1622 is an abnormal inscription in that, though we possess 783 lines of it, it contains no general lists of ships or equipment handed over, but is confined almost entirely to debts paid. These are organised in three sections. First (lines 1–378), debts collected from trierarchs arranged by tribes. The list begins in the middle of tribe VII. Tribes VIII–X occupied almost exactly two columns. I–VII will have occupied, roughly, rather more than four, which will make the whole text very large even without lists of ships and equipment. A second section (lines 379–579) is headed τάδε παρὰ τῶν ἀρξάντων ἐν τοῖς νεωρίοις εἰσπέπρακται in the four archonships 345/4 to 342/1; the former officials are again arranged by tribes. Thirdly (lines 580–783) we have a section arranged by ships. In this section, some non-payments of debts are recorded, but, except in lines 723–36, for which I cannot account, only in conjunction with actual payments made by people who shared the responsibility with the non-payer.[17] Thus the very large section of this list which we have, demonstrably once even larger, only contains entries of a purely financial nature.

We therefore have a gap in the normal record of the *neorion epimeletai*

[16] M. G. Clark, 'The Economy of the Athenian Navy' 78–80, follows D. R. Laing, *Hesperia* 37 (1968), 245 n. 4, in dating *IG* ii² 1620 + 1621 to 348/7.

[17] What is the distinction between the third section and the first? Since Timotheos, who died in 354, is recorded as paying a debt in the third section (lines 632, 636), Boeckh, *Urkunden über das Seewesen* 359, held that the first section referred to debts collected after 345, the third section to earlier collections. This does not seem very plausible, and a more fundamental distinction seems to be that in the first section triremes are never referred to as held by more than three trierarchs, with double trierarchies and even singles much more common, whereas in the third section, again with the exception of lines 723–36, those jointly responsible are in fives, sixes and sevens. It seems simpler to hold that the debts paid in the first section are older than the symmory law of 357, and were more easily booked to individuals, and that those paid in the third section were more recent and more easily booked by ships. To suppose that the third section recorded payments made before 354 is more difficult than to suppose that payments made by Timotheus' heirs on his behalf were simply booked as made by him (cf. lines 587–8, where a similar confusion was made, but this time detected).

Clark, 'Economy' 111–21, finds difficulties in the supposition that debts in the first section are less recent and debts in the third more recent than the symmory law, and suggests that the debts in the first section were finally cleared and the debts in the third were not. He points out *per epistulam* that there is no indication of what was in the deleted lines 587–8.

from 347/6 to 335/4 inclusive, a gap which may or may not be due to the chances of discovery. Can we say that *IG* ii² 1622, though abnormal, is nevertheless a record of the *epimeletai*? Let us first consider its negative light on the dockyard-officials for 347/6 and onwards. Its list of officials paying debts is complete for six tribes and has part of two more; only Pandionis and Leontis are totally unrepresented. We have already noted that it provides evidence for a *neorion epimeletes* for 348/7 (lines 549–52);[18] it does not provide evidence for an *epimeletes* for any later year. For 347/6 it records a ταμίας ἐς τὰ νεώρια (lines 444ff.) This official is otherwise only known for 377/6 (*IG* ii² 1622.436–7, somewhat dubious)[19] and 323/2 (*IG* ii² 1631.509). In this second case his activities, as far as we can see, are to receive money; in the first case, if it stands, he may have some responsibility towards *skeue*, though this is by no means certain. However, in 347/6 the functions of the *tamias* are certainly concerned with *skeue* and seem such as to preclude the existence of the *neorion epimeletai*. His debt is τῶν σκευῶν [ὧν] ἔλαβε παρὰ [τῶν] τριηράρχων [καὶ οὐκ] εἰσήνενκε γρά[ψας] ἐν τῆι στήληι. He collected *skeue* from the trierarchs, he drew up the *stele*. We would have thought that both of these were normal (cf. 1631.410–15) functions of the *neorion epimeletai*.[20] οὐκ εἰσήνενκε, not οὐ παρέδωκε, is another oddity. We have another official for 346/5 (lines 420 ff.), an even more abnormal one, simply described as αἱρεθεὶς ἐκ βουλῆς; he also seems to have been acquiring *skeue*. One more official of 346/5 is mentioned (lines 387–97): the ταμίας τριηροποιϊκῶν of 346/5 is said to have received 1,800 oars παρ' ἡμῶν, presumably in that year. This raises directly the question of who drew up the document and the length of their tenure. We have already observed that the heading of the second section (lines 379–85) covers four years, but the word employed is εἰσπέπρακται and not εἰσεπράξαμεν, so that we cannot infer from that that the board had been in office for the four years 345/4 to 342/1.[21] This document was drawn up in 342/1 at the earliest. I do not know any other instance of an Attic board describing their predecessors as ἡμεῖς, and on the face of it, lines 387–97

[18] He pays 1,637 dr. 1 ob., a very large sum for such an official.

[19] The restoration of this entry has now been criticised as too long to fit the space by Clark, *BSA* 85 (1990), 48–9 (cf. 'The Economy of the Athenian Navy' 113 with n. 188), who following Lewis suggests a reference to the archon of 363/2.

[20] Clark *per epistulam* stresses that he collected tackle from dozens of ships and his fine was huge: the great scale and scope of his activities suggest that there may have been no *epimeletai* in 347/6.

[21] Note that this is not a Panathenaic period.

should mean that the board producing this document has been in office for five years.

The implications of this cannot be probed fully on present evidence. The evidence of *IG* ii² 1622 would seem to point to something abnormal beginning to happen to the department of the *neoria* in 347/6. Besides Aesch. III.25, two other passages are relevant to this period. The first is the entry νεῖα καινά: ΔΔΠ ὧν Εὔβουλος ἐπρίατο from various lists of the twenties (*IG* ii² 1627.353, 1628.524, 1629.1000, 1631.231). Such an entry is paralleled only by similar purchases by Demades, apparently in 325/4 (*IG* ii² 1629.347, 697), and ought to indicate an exercise of individual responsibility, outside the normal working of state boards, and a special commission for Eubulus. Such a special commission is in fact attested for Demosthenes in 340 by Aesch. III.222, σαυτὸν πείσας Ἀθηναίους ἐπιστάτην τάξαι τοῦ ναυτικοῦ, although Aeschines gives no details except on Demosthenes' trierarchic law.[22]

Aesch. III.25 goes on to say that οἱ ἐπὶ τὸ θεωρικὸν σκευοθήκην ᾠκοδόμουν. *IG* ii² 1668 throws no light on who was constitutionally responsible for this, but it must be noted that *IG* ii² 505.11–17 gives grounds for associating the *skeuotheke* even more closely with the *neoria* than their actual relation would suggest, in that the two metics there praised εἰς πολλὰ τῶν [συ]μφερόντων τῶι δήμωι χρήσιμοι γεγόνασιν, εἵς τ[ε] τὴν οἰκοδομίαν τῶν νεωσοίκων καὶ τῆς σκευοθή[κ]ης εἰσφέροντες τὰς εἰσφορὰς καθ' ἕκαστον τὸν ἐ[ν]ιαυτὸν τὰς εἰς τὰ δέκα τάλαντα καλῶς καὶ προθύ[μ]ως ἀπὸ Θεμιστοκλέους ἄρχοντος μέχρι Κηφισοδ[ώρ]ου (347/6–323/2).[23] Their contributions to this special fund may have begun in 347/6 because their residence in Athens began at this time, but it is more likely that the normal inference is the correct one, and the fund itself and the building began in 347/6. It seems highly probable that the beginning of building works in the dockyards is to be connected with the disturbance in the routine of the dockyard-officials which we have seen reason to believe begins in that year.

Of ἦσαν δὲ καὶ ὁδοποιοί there is nothing to say.

It can therefore be said that, in two cases, that of the *antigrapheus* and that of the *neorion epimeletai*, there is some evidence of change in the period under review. I am therefore reluctant to agree with Jones that Aesch. III.25

[22] Rhodes, *The Athenian Boule* 239–40, briefly followed Lewis on the temporary disappearance of the *epimeletai*.

[23] Some have connected with the *skeuotheke* an assembly περὶ τῶν ἐν τοῖς νεωρίοις late in 347/6 (Dem. XX.60); but such assemblies are not infrequent.

is 'mere rhetoric'.[24] But it must be confessed that our positive evidence is very small, and that we lack entirely any means of discovering how οἱ ἐπὶ τὸ θεωρικόν actually worked in the 340s. I said earlier that there was no means of deciding whether iteration of the office was possible at that time. Now the evidence of *IG* ii² 1622.387ff. has offered the possibility that there was a board during this period, presumably οἱ ἐπὶ τὸ θεωρικόν, though a special board of ἐπιστάται τοῦ ναυτικοῦ cannot be excluded, which actually served a five-year term, and we cannot begin to guess how this would have worked. Did it submit to *euthynai* every year?

II LYCURGUS

There is one clause of Aesch. III.25 which we have so far not examined, which indeed has hardly been examined at all. These conditions obtained πρὶν ἢ τὸν Ἡγήμονος νόμον γενέσθαι. It is clear that Demosthenes held the office before this law. It is equally clear that he was in office in spring 336. The law of Hegemon therefore belongs at the earliest to the summer of 336. It is necessary for Aeschines' case that the prerogatives of οἱ ἐπὶ τὸ θεωρικόν, as outlined, should not have been infringed or taken away before the summer of that year. But this is at variance with all forms of the universally held theory that Lycurgus assumed control of Athenian finance with some title or commission at some time during 338 and held it ἐπὶ τρεῖς πενταετηρίδας until 326.[25] There is no specific evidence for this date, with the exception of a passage of Hyperides which demands further examination. It rests entirely on the belief that any *pentaeteris* must be Panathenaic. Since it conflicts with the evidence of Aeschines, the year 338 must be abandoned.

Is the year 326 any more suitable for the terminal date? Firstly, when did Lycurgus die? I have shown grounds for believing that he went on the Pythais to Delphi on the occasion of the rededication of the Delphic temple in late summer 326.[26] He appears nowhere in the Harpalus affair, though [Plut.] *X Or.* 848F is the only explicit evidence that he was dead; that makes

[24] *Athenian Democracy* 129.

[25] ἐπὶ τρεῖς πενταετηρίδας [Plut.] *X Or.* 841B, cf. decree *ap.* 852B, Diod. Sic. XVI.81.1. 338–326: e.g. Beloch, *GG*², iii.1.611; W. W. Tarn, *CAH* vi¹.440–1; Glotz with Cohen, *HG* iii.366. Nineteenth-century writers assumed that the choice lay between 342–330 and 338–326: e. g. Grote, *HG* (12-vol. ed.) xii.101 n. 1 = (10-vol. ed.) x.217 n. 1.

[26] *BSA* 50 (1955), 34; but note Lewis's reservations in *Hesperia* 37 (1968), 377–8 n. 29.

early summer 324 a *terminus ante quem*.[27] His remark about Alexander's deification (*X Or.* 842D) could have been made at any time. The Aristogeiton case (Dein. II.13) has been put in 325/4, but might be moved back a year or so.[28] On the evidence he could have died at any time between late summer 326 and early summer 324. Did he complete his third term? The reliability of the story in *X Or.* 842F is not, I suppose, beyond doubt. If it is true, his wish to give *euthynai* on his death-bed[29] would suggest very strongly that he was still in office. The conventional picture which makes Menesaichmos succeed him in 326 (μετὰ Λυκοῦργον τὴν διοίκησιν τῶν δημοσίων χρημάτων παραλαβών, Dion. Hal. *Dein.* II (660 U&R)) makes nonsense of a story of Menesaichmos attacking these death-bed *euthynai* well after the changeover. Stronger evidence comes from the admitted fact that his sons were imprisoned after his death for claims made against him: ὑπὲρ ὧν τὸν πατέρ' αἰτιῶνταί τινες, τοὺς υἱεῖς δεδέκατε (Dem. *Ep.* iii.13 and the whole passage, well informed even if not by Demosthenes; cf. *X Or.* 842E, Hyp. fr. 118 Jensen = Kenyon). This is at least very strong evidence that he had not finished his last term of office and gone through *euthynai*. Again, the supposition that Lycurgus had a twelve-year term from 338 to 326 proves unsatisfactory.

We must turn to the Hyperides passage which has assumed great importance both for the dates of Lycurgus' term of office and for its name. This is *Dem.* 28ff. Starting in a lacuna it runs, in current texts,

> οὐ μέντοι] ἡμᾶς ὁ δῆμ[ος ἐν τῷ] μετὰ ταῦτα χρ[όνῳ] οὐκ εἶα προσ[ιέναι] αὐτῷ οὐδὲ δια[λέγε]σθαι, ἀλλὰ καὶ συ[μβούλοις ἐχ]ρῆτο καὶ σ[υνηγόροις. τότε δὴ Λυκοῦργον προστάτην μᾶλλον εἵλετο· τοῦ δ]ὲ ἐπιόν[τος μηνός, ἐ]πὶ τὴν δι[οίκησιν τῶ]ν αὐτοῦ ἅπασαν [ταμ]ίαν ἐχειροτόνησ[εν, ὑπ]ολαμβάνων χάριν αὐτῷ παρ' ἡμῶν ὀφείλεσθαι· ὅπερ δίκαιον ἦν.

A reference to Lycurgus, whose name will have been swallowed in the two-line lacuna, was first seen here by Blass, and has never been questioned as far

[27] Davies, *A.P.F.* 351, influenced by this paper, dates his death 325/4.

[28] 324, Hansen, *Apagoge, Endeixis* and *Ephegesis* 142; 325/4, Develin, *Athenian Officials* 403-4.

[29] Only τῶν πεπολιτευμένων in [Plut.], but cf. *IG* ii² 457.*b* 21–2 δ]οὺς εὐθύνας πολλάκις [τῶν τε πεπολιτευμένων καὶ τῶν] διωικημένων for the implicit connection; τῶν διωικημένων has also dropped out of the version of this text transmitted in *X Or.* 852B–C.

as I know. Further progress was made by Jensen, who read τοῦ δ]ὲ ἐπιόν[τος for Blass's [π]λείστων which provided no acceptable sense, and by Kenyon, who proposed the restoration [ταμ]ίαν, which seems certain. The historical results of these last two suggestions were clarified by Colin, who argued that Kenyon's restoration proved that ταμίας was an element in Lycurgus' title, comparing *X Or.* 841B and the decree *ap.* 852B, and that the precise title was ταμίας ἐπὶ τὴν διοίκησιν.[30] This has been fairly freely accepted,[31] and argument has mainly been about what to restore after τοῦ δ]ὲ ἐπιόν[τος --. Colin at first read ἔτους with Jensen, which would imply that Lycurgus' term started in 337 (that μετὰ ταῦτα refers to Chaironea is not, I think, to be disputed). This offended against the long-cherished 338–326 view, and in his edition of Hyperides Colin went over to μηνός.[32] This retains 338, but implies a start some two months after the Panathenaea. To retain a start at the Panathenaea with Ferguson means discounting Hyperides.

This is all very well, but what has it to do with the speech? The theme at this point is the trust placed by the *demos* in anti-Macedonians in general and Demosthenes in particular. Why should Hyperides choose as his only example of this the trust placed in Lycurgus? Why should Lycurgus be singled out as a person to whom the *demos* owed gratitude? How does this tend to show Demosthenes' ingratitude to the *demos*? It could be maintained, more particularly, that this exaggerates Lycurgus' importance as an anti-Macedonian before Chaironea, for which the evidence is rather slight. There is the embassy to the Peloponnese in 343 attested by [Plut.] *X Or.* 841E and the longer version of Dem. IX.72. The decree of 307/6 is remarkably silent about this activity, and there may not have been much of it (cf. Dem. *Ep.* iii.2: περὶ τῶν Ἑλληνικῶν καὶ συμμαχικῶν οὐδὲν εἰωθὼς γράφειν.)

Can we make better sense of Hyperides? It seems to me that we can, quite simply. If the reference is not to Lycurgus' twelve-year tenure, but to Demosthenes' tenure of the office ἐπὶ τὸ θεωρικόν in 337/6, the problems all vanish. Trust in Demosthenes is proved by citing his election to this office. Without making more than a guess, σ[υνηγόροις· τοῦτον γὰρ ἐπὶ τὸν λόγον τὸν ἐπιτάφιον εἵλετο, τοῦ δ]ὲ ἐπιόν[τος ἔτους κτλ, fits the facts and fits the speech. This office, which we know he held, is the *charis* which the

[30] *REA* 30 (1928), 189–200.

[31] E.g. Ferguson, *Treasurers of Athena* 139 n. 2.

[32] *Hypéride: Discours* (Budé), pp. 222–3, influenced by P. Treves, *REA* 36 (1934), 513.

demos owed Demosthenes for his campaign against Philip. If Aeschines could say that the holders of this office σχέδον τὴν ὅλην διοίκησιν εἶχον τῆς πόλεως, Hyperides could say that Demosthenes was elected ἐ]πὶ τὴν δι[οίκησιν τῶ]ν (τοῦ δήμου) ἅπασαν [ταμ]ίαν. ταμίας is not, I think, strictly accurate technically, but I do not think this a serious obstacle.

We have now eliminated Lycurgus from the period before 336, and we must return to the consideration of what happened in that year or shortly afterwards. Firstly, the power attaching to οἱ ἐπὶ τὸ θεωρικὸν was broken by the law of Hegemon. This law is not, I think, to be confused with the law of Hegemon referred to in *IG* ii² 1628.297–300.[33] All we know of that is that under its provisions the *boule* of 326/5 was empowered to sell off surplus *skeue*. This may have been a clause in a law passed between 336 and 330 which affected the powers of οἱ ἐπὶ τὸ θεωρικόν, but it might equally well not have been. A law which broke the powers of οἱ ἐπὶ τὸ θεωρικόν might have been couched in various forms. It could have restricted their rights specifically, it could have recreated various magistracies which had disappeared under Eubulus, it could have created a new superior magistracy such as that held by Lycurgus, or it could have imposed some cramping restriction on tenure which made it impossible to use the office for effective financial control of the state. The first two possibilities are not improbable. I have been attracted by the third, but would abandon it for two reasons. It is clear that Hegemon was consistently pro-Macedonian (see the passages collected in *PA* 6290) and I doubt whether he would be confident of his ability to exclude an anti-Macedonian from this new position. I doubt specifically whether he would have welcomed the appointment of Lycurgus. Secondly, if the new position held by Lycurgus was created by a law, I find its absence from *Ath. Pol.* very odd.

The fourth possibility is more interesting. We have already noted that it is by no means certain that the office of οἱ ἐπὶ τὸ θεωρικόν could not be held repeatedly in the 340s, and that there is some possibility that some financial commission went on for at least five years in that period. Lycurgus, however, when entering on his second period of office was forced to substitute a nominee, διὰ τὸ φθάσαι νόμον εἰσενεγκεῖν, μὴ πλείω πέντε ἐτῶν διέπειν τὸν χειροτονηθέντα ἐπὶ τὰ δημόσια χρήματα ([Plut.] *X Or.* 841c). This is dateless, but I had long been thinking of it in connection with Hegemon

[33] Contr. Cawkwell, *CQ* n.s. 19 (1969), 169 n. 2.

before I realised that there were strongish grounds for importing his name into the text. As the text stands, Lycurgus passed the law himself. This is not very probable, and Reiske long ago suggested that a name had dropped out.[34] If a name has dropped out, I doubt whether any reading will be found to compete with διὰ τὸ φθάσαι ⟨Ἡγήμονα⟩ νόμον εἰσηνεγκεῖν for factual and palaeographic probability.[35] Tentatively, then, I would suggest that Hegemon, struck by Demosthenes' election to οἱ ἐπὶ τὸ θεωρικόν and contemplating the possibility that the office might be used by him in the interest of his policy as Eubulus had used it in the interests of his, may have forbidden iteration, thereby reducing its tenure to the position found in *Ath. Pol.*, and have forbidden any irregular financial commission, e.g. over the fleet, to last πλείω πέντε ἐτῶν. It is, I suppose, possible that οἱ ἐπὶ τὸ θεωρικόν had previously had no fixed tenure. This would explain why Aeschines calls for evidence of the year, month and day when Demosthenes was elected, which seems superfluous if tenure was regular. My references to Demosthenes' tenure for 337/6 would then be slightly modified.

This is very hypothetical, but it seems to be effective politics. I think it likely that it happened some time in 336, since we have to find room for Lycurgus to have a reasonable portion of his third *pentaeteris*. Hegemon's law went through, and those who believed in the principles of financial continuity had to find a new method of operation. This consisted of giving Lycurgus special commissions for four years at a time. The task of distinguishing these and giving them titles on the basis of [Plut.] *X Or.* 841B–C, 852B–C has given many different results,[36] and it should not be re-attempted until the epigraphical material has been further studied. The decree for Lycurgus *ap. X Or.* 852B–C is not reliable evidence. Not only does it diverge

[34] *Plutarchi . . . cum notis G. Xylandri et I. I. Reiskii* ix (Leipzig: Georg, 1778), 346 n. 71: *Quis legem illam tulerat? Nomen excidit. Si ipse Lycurgus legis auctor fuit, potest oratio integra esse.*

[35] S. S. Markianos, *GRBS* 10 (1969), 325–31, suggested that διὰ τὸ φθάσαι νόμον εἰσενεγκεῖν means 'in order to forestall the introduction of a law'. Rhodes thinks this extremely improbable, and has no reason to suppose that Lewis thought otherwise.

[36] These start with U. Koehler, *Hermes* 1 (1866), 316. Many have assumed that what was forbidden was simply more than four years' consecutive service, and that Lycurgus served in person again in the third *pentaeteris* (e.g. Davies, *A.P.F.* 415), but Rhodes thinks there was an absolute maximum of four years and Lycurgus served in person in the first *pentaeteris* only (*The Athenian Boule* 107; cf. earlier B. D. Meritt, *Hesperia* 29 (1960), 4).

from what is left of the version on stone (*IG* ii² 457),[37] but, even if we had a reliable copy, it would still only reflect what the restored democrats, notably Stratokles and Lycurgus' son, Lykophron, wanted recorded in 307/6. Its tendency to glide over constitutional difficulties is sufficiently evidenced by its assertion that he was τῆς κοινῆς προσόδου ταμίας for three *pentaeterides*, without the qualification about the nominee recorded in 841B. The only firm evidence for Lycurgus' powers, apart from what he might propose in the assembly, is *IG* ii² 1672.11, from which we learn that in the first prytany of 329/8 he was in a position to order (Λυκούργου κελεύσαντος) the *epistatai* of Eleusis and the Treasurers of the Goddesses to pay their architect in advance.

I must, however, briefly discuss two inscriptions lest they should be used as a weapon against the view here advanced that Lycurgus' three *pentaeterides* were 336–332, 332–328, 328–324. Firstly, *IG* ii² 1496. The accounts of this board, at any rate as far as they concern the *dermatikon*, begin in the sixth month of 334/3, and continued at least to the ninth month of 331/0. They could have finished with the archon-year 331/0, or with the Panathenaea of 330, or have continued a full four years to the sixth month of 330/29. I will show elsewhere that the obverse of the inscription is concerned entirely with their receipts during this period, the reverse with their products, the κόσμος κανηφορικός of [Plut.] *X Or.* 852B.[38] Lycurgus' name may appear on a mysterious fragment *IG* ii² 413, which could belong to the top of the stele. However, this does not fit Koehler's view that Lycurgus was ἐπιστάτης τῶν ἱερῶν for the Panathenaic period 334–330, for (1) the accounts were presented by a board and not by an individual (cf. lines 61, 200); (2) the operations of the board only began five months after the Great Panathenaea of 334. It is likely that they completed their operations in time to display their *kosmos* at the Great Panathenaea of 330, but that was the obvious and necessary time, which takes no account of financial administration.

In any case, the activity described in *X Or.* 852B cannot be confined to 334–330. The inscription *IG* ii² 1493 has generally been thought to be parallel to ii² 1496, and to account for the construction of the *nikai*. Further study

[37] For a convenient presentation of the text in parallel columns see the Teubner ed. of Lycurgus by N. C. Conomis, pp. 13–17.

[38] This will appear in a reworking by Diane Harris of scattered unpublished material by A. M. Woodward and Lewis.

shows that 1493 cannot belong to 334/3, for the evidence of *IG* ii² 336 is incompatible with its prytany dates, and four further fragments, *IG* ii² 1494, 1495, 1497 and *Hesperia* 6 (1937), 456–7 no. 6, belong to the same stele.[39] *IG* ii² 1497 records operations covering 327/6. Again, these are accounts of a board, more precisely, two boards, and even if they could be shown to cover a Panathenaic period, they would still not be relevant to Lycurgus.

III ADDENDA 1993

With some necessary modification, the foregoing represents the version which has had considerable esoteric circulation. It originally continued with an attempt to identify the honorand of *Hesperia* 10 (1941), 42 no. 10, as Lycurgus and to recover from that decree some contemporary evidence on his position and titles. A major factor in putting the paper back in the drawer was the discovery of the top half, *Hesperia* 29 (1960), 2–4 no. 3 = *SEG* xix 119, which showed that the decree referred to Xenokles of Sphettos.[40] There are nevertheless points to consider relevant to the Lycurgan period, and we must look at the text:

[. . . . [10] . . . .]ς εἶπεν· δε-
[δόχθαι Κήρυ]ξιν, ἐπειδ-
[ὴ Ξενονλῆς Ξ]ειν[ί]δος Σ-
[φήττιος ἀνή]ρ ἐσ[τ]ιν ἀ[γ]-
[αθὸς περὶ τὸ] γέν[ος] τ[ὸ Κ]-
[ηρύκων, ποιῶ]ν [ἀεὶ ὅ] τ[ι ἂ]-
[ν δύνηται ἀ]γαθόν, κ⟨α⟩τ[α]-
[σταθείς δ' ἐ]πὶ τῆι διοι-
[κήσει τῆς π]όλεως καλῶ-

[39] F. W. Mitchel, *TAPA* 93 (1962), 213–29, with *corrigenda AJA* 70 (1966), 66, argued that *IG* ii² 1493 does after all belong to 334/3 and the restoration of the tribe in prytany in *IG* ii² 336 is wrong. His reinterpretation is rejected by Osborne, *Naturalization* i.75–6 (D23).

[40] Lewis does not here discuss the date of the inscription. Xenokles was active from 346/5 to 306/5, but Lewis agreed with Meritt in judging the lettering to be of the Lycurgan period (*ap.* Rhodes, *The Athenian Boule* 108 n. 1).

[ς καὶ εὐσεβ]ῶς ἐμέρισε
[ὅπως ἦι τὰ ἱ]ερὰ θῦσαι [τ]-
[ὸ γένος τὸ Κ]ηρύκων ὑπ[έ]-
[ρ τε τοῦ δήμ]ου τοῦ Ἀθην-
[αίων καὶ ὑπ]ὲρ τοῦ γέν[ο]-
[ς τοῦ Κηρύκ]ων· ἐπαινέ[σ]-
[αι αὐτὸν κα]ὶ στεφανῶσ-
[αι χρυσῶι στ]εφάνωι ἀπ-
[ὸ χιλίων δ]ραχμῶν καὶ ε-
[ἶναι πρόσο]δον αὐτῶι π-
[ρὸς τὸ γένος τ]ὸ Κηρύκ[ω]-
[ν---------] Λ[. . . .]

I had noted the similarity with one of the genuine contemporary references to Lycurgus' position, Hyp. fr. 118 Jensen = Kenyon: οὗτος ἐβίω μὲν σωφρόνως, ταχθεὶς δὲ ἐπὶ τῆι διοικήσει τῶν χρημάτων εὗρε πόρους κτλ. I was looking for a participle in what are now lines 7–8, but had not found καταστα θείς. A further point of importance lies in ἐμέρισε. The official had been performing the (or a) *merismos*. Simply passing out the funds would be the normal function of the *apodektai*. It seems more likely, I think, that he has been reorganising the *merismos*, so that proper funds shall be available for the Kerykes. I am not certain whether we should read, with Meritt, ἐμέρισε[ν τὰ εἰς τὰ ἱ]ερὰ θῦσαι, or take ἐμέρισε as intransitive here as in Arist. *Pol.* II.1268B15, though in rather a different sense, restoring, say, ἐμέρισε [ὅπως ἦι τὰ ἱ]ερὰ θῦσαι. In any case, there is no parallel for the language; we are dealing with an extraordinary office.

There are now treatments of Xenokles by Davies and Ampolo.[41] They see a further reference to his tenure of this office in *IG* ii² 1191.23, καὶ δ[η]μόσια διαχειρίσας χρήματα πρότερόν τε καὶ νῦν ἐπὶ δικαιοσύνει, and agree that it is most likely that he was the friend of Lycurgus who had to be imposed between his tenures of office. But the situation is still far from clear; consider once again *IG* ii² 1672.11, with its evidence that Lycurgus himself had an abnormal position in 329/8. This and what is said of Xenokles still constitute the main evidence about the way his functions worked.

I prefer not to go further into his functions here. The enquiry has so far

[41] Davies, *A.P.F.* 414–15; C. Ampolo, *PP* 34 (1979), 167–76.

been confined as far as possible to what Andreades described as the archaeological problems, titles and dates.[42] His work and that of Francotte on Athenian budgetary theory[43] stand in need of revision,[44] but the problems treated here seem to stand in need of this preliminary investigation.

[42] *History of Greek Public Finance* i.373 n. 1. [43] *Les Finances des cités grecques* 219–33.

[44] Rhodes, *The Athenian Boule* 105–8, 235–40, *Commentary on the . . . Ath. Pol.* 313–17, was influenced by this paper but did not agree with it at all points (cf. some of the notes above). The most thorough recent treatment of these questions is Faraguna, *Atene nell' età di Alessandro* 187–94 (stratiotic and theoric funds), 195–209 (Lycurgus); see also J. Engels, *Anc. Soc.* 23 (1992), 5–29, esp. 5–15; H. Leppin in Eder (ed.), *Die athenische Demokratie im 4. Jahrhundert v. Chr.* 557–71.

26

On the dating of Demosthenes' speeches

It would be pointless to deny that this paper is to some extent a salvo in an exchange of fire which I have been conducting on and off with George Cawkwell for fifteen years or more, but of which very little more than the opening shots have ever got into print. Very early in life I suggested dates for Dem. XXII and XXIV which diverged from those given in the *Ad Ammaeum* of Dionysius of Halicarnassus.[1] Some reply to my specific points was given in an article by Raphael Sealey, the last complete survey of the dates in that work,[2] and Cawkwell also landed some well-aimed shots (I had in particular failed to read an *apparatus criticus*).[3]

On the general point at issue Cawkwell warned us that the information available to us was scanty, and that, since we lack the means to test the accuracy of Dionysius' dates, we should resist the temptation to reject them as worthless. 'Both the general histories and the *Atthides* and many lost speeches were available to Dionysius and his predecessors, and the dating of Demosthenes' speeches by means of internal evidence alone must have been far easier for him than for us.' He cites various specific cases of the kind of evidence which might have been available and concludes, 'When such evidence was available to Dionysius, only for cogent reasons should his dating be rejected.'[4] Sealey had thrown the emphasis further back. Holding, in

* A paper read to the Oxford Philological Society on 23 January 1970. There is a survey of work on Demosthenes between 1915 and 1965 by D. F. Jackson and G. O. Rowe, *Lustrum* 14 (1969) (published 1971). Thanks are due to Dr J. C. Trevett for his help with the notes to this paper.

Citations from the works of Dionysius of Halicarnassus are by sections and by pages and lines in the Teubner edition of Usener and Radermacher. *Philippics* in italic type are the speeches which we today call the *Philippics*; Philippics in roman type are the speeches called Philippics by ancient writers.

[1] *BSA* 49 (1954), 32, 39–49. Agora I 7495, to be published in *Hesperia* by J. M. Camp and M. B. Richardson (cf. meanwhile Develin, *Athenian Officials* 287), supports the proposed date of 354/3 for XXIV.

[2] *REG* 68 (1955), 77–120. [3] *C&M* 23 (1962), 34–49. [4] *Ibid.* 40, 41.

my belief, correctly, that the dates in *Ad Amm.* are not Dionysius' own, he reminds us that the dates had probably been determined by Alexandrian scholars and could command the respect of a lettered, if not a learned public. Sealey indeed did find the information in *Ad Amm.* not free of error, and my own feeling about his article is that what he was saying was that the dates in *Ad Amm.* were right except when they were wrong. What separates me from Cawkwell and Sealey, I think, has always been a question of confidence. How far should we believe in those statements which are unsupported? It is to this question I wish to direct attention.

I am emboldened to reopen the question by recent operations on Alexandrian scholarship by K. J. Dover, both on the *Clouds* and on Lysias. You will recall that though he did not in the least underestimate, and nor would I, the task facing Callimachus as he came to the job of putting classical literature in order, he did come to the conclusion on the *Clouds* that Callimachus

(1) had read the play so carelessly that he did not realise that he was dealing with a revised play.

(2) preferred to assume an error in the records before him rather than abandon an erroneous view.

(3) was ignorant of the existence of an unrevised text which certainly existed all the time.[5]

With reference to Lysias, after a discussion of the evidence about ancient conflicts on ascription, Dover could certainly rightly feel himself entitled to say 'We are trying . . . to do what Kallimachos had not the time nor equipment to do; to do afresh what Dionysios and Caecilius were the first to attempt on a significant scale. The evidence available to Dionysios was far greater in quantity than that available to us, but we are, after all, two thousand years older, and experience may have taught us something more about the use of evidence.'[6] It seems to me that, for dating Demosthenes' speeches, our task is the same. Why this is so, I shall be trying to explain. My opponents lie far further back than Cawkwell or Sealey.

I begin with Dionysius himself, and not with Demosthenes at all but with the *Deinarchos*, where we have a far fuller picture of Dionysius at grips

[5] Dover (ed.), *Clouds* lxxx–xcviii, esp. xcvii–xcviii.

[6] *Lysias and the Corpus Lysiacum* 23–7, esp. 26–7.

with the sort of problems which concern us here. The treatise has sometimes been very warmly regarded. I have heard one of our most revered ancient historians commend it for the rigour of its scholarly method.[7] Such praise seems to me exaggerated, and I hope my reasons for thinking so will emerge. A look at it will illuminate both Dionysius' methods when left to himself and the normal resources on which he would expect to draw.

He is here left to himself, as he says with considerable emphasis. Neither Callimachus nor the Pergamene *grammatikoi* have written anything ἀκριβές about this, and through failure to examine facts have fallen short τῶν ἀκριβεστέρων, so that, apart from other mistakes, they have given Deinarchos speeches which are not his and attributed some of his speeches to others (eventually he only gives one instance of the last mistake). The only other authority at which he admits to having looked is Demetrios of Magnesia's περὶ τῶν ὁμωνύμων. One might have expected ἀκριβεία there, but in Demetrios' whole treatment, which he quotes in full, he finds nothing ἀκριβές or true (§§1–2 *init.* = 297–299.10). Dionysius then proceeds to what he has been able to find out himself. The first section of this (299.15–301.20) depends largely and rightly on Deinarchos' own speech against Proxenos, spoken towards the end of his life, which he has evidently read with care. If one wonders whether some of it comes from any other source, particularly towards the beginning, there is a serious possibility for further information in that some of it sounds to me quite like Hermippos, the Callimachean, of whom more later. He is elsewhere (*Isaios* §1: 93.14) described by Dionysius as ἀκριβὴς ἐν τοῖς ἄλλοις γενόμενος, and is evidently a person to whom one would turn for biographical information about orators. In fact, if you compare the opening of the *Isaios*, from which that remark comes, with Harp. *s.v.* Ἰσαῖος, it becomes evident that there Dionysius had consulted both Hermippos and Demetrios, but only names Hermippos. Here he only names Demetrios, and a use of Hermippos is clearly at least possible. The information acquired from Deinarchos himself and possibly Hermippos is then gratifyingly thickened in a second section (§3: 301.21–302.21) of quotations from Philochorus, again work which he has done for himself.

He now turns to the question of establishing authenticity. The criteria to be applied are chronological and stylistic. To reverse his order and start with style, I think it fair to say firstly that he misses one fairly obvious procedure,

[7] A. Momigliano: see *RSI* 87 (1955), 17–46 at 34 = *Sesto Contributo* i. 33–67 at 53.

the procedure followed by Dover with Lysias. He could have said, just as Dover did with Lysias XII, that, if any speech was certainly by Deinarchos, it must be the speech he himself delivered against Proxenos, which he has already used as the main authority for the life. This never crosses his mind. Instead, he treats the fact that a speech bears the name of Deinarchos as raising a presumption of authenticity, a pretty feeble presumption, as he knows, and develops negative criteria by which he can refuse to Lysias, Demosthenes and Hyperides speeches in the Deinarchean corpus which resemble them. If these negative criteria are present, they can stay Deinarchean and be regarded as γνήσιος, assuming that they pass the chronological criterion. He admits that it is difficult to establish possible Deinarchean criteria, with the result (§9: 309.15–16) that τοῖς γνησίοις τὸ τῆς ἀναγραφῆς προσέσται μόνον. I confess to not being particularly impressed by this.

Now for chronology. His fundamental move (§4: 303.2) is to assume that Deinarchos was seventy in 292/1, because he calls himself γέρων. Reckoning roughly, τὸ γὰρ ἀκριβὲς οὐκ ἔχομεν, that will bring his birth-year out at 361/0. If anybody says he was older or younger than that, he will not only be saying οὐδὲν ὑγιές, but he will be robbing him of all his speeches but five or six, by making him too old for some and too young for others. I cannot attach any precise meaning to this sentence at all, I fear. He then assumes that we will not be wrong to assume that Deinarchos started to write at the age of twenty-five or twenty-six. The 26th year will be 336/5, so we should reasonably distrust any speech earlier than that year. He then rules out speeches made between 307 and 292, on the entirely reasonable ground that Deinarchos was in Chalkis during this period and it is unlikely that anyone would have sailed to Chalkis to get a speech. I say nothing further of arguments about this later period, but it is clear that there is something mildly arbitrary about the choice of 336/5, and that raising Deinarchos's age in 292/1 above seventy or lowering the beginning of his activity might start to bring in speeches going at any rate some way back into the 340s. By a not unfamiliar process, also, hypothesis starts to slide into fact, so that the birth-year eventually gets described in such firm language as οὕπω such and such an ἔτος ἔχοντος αὐτοῦ (§11: 313.1–2 and 5, 314.9, §13: 319.14–15, 320.19–20, 321.5), and καθ' ὃν εὑρίσκεται γεγεννημένος (§13: 319.20). The language is strong and firm, and there is no trace of any feeling that the argument, strong for speeches of the 350s, is going to be progressively less soundly based as we get on into the forties.

The use of the chronological argument depends, further, on Dionysius' ability to read the speeches of Deinarchos accurately and draw the right chronological conclusions. On this we can get some check from the extent speeches. The three we have under Deinarchos' own name would present no chronological difficulty, since they are all on the Harpalos affair, but I must say I don't understand why Dionysius, who has already reported that Demetrios of Magnesia denied the authenticity of the speech against Demosthenes (§1: 298.20–299.4), offers no specific defence of it. Three of Dionysius' list survive in the Demosthenic corpus, and we can form our own views. The *Theocrines* (Dem. LVIII) he lists as γνήσιος, though noting that Callimachus listed it as Demosthenic. It has always been disquieting, or should have been, that Callimachus could ascribe to Demosthenes a speech 'which contains plainly hostile and contemptuous references to Demosthenes himself'.[8] But what of Dionysius? Not only does he not give this argument against Callimachus, but, if he applied his own criteria, he ought to have been worried. The archon of 344/3 is mentioned in §28. He is evidently a little time back, but that it is not all that long ago emerges from §§37–8, where the defection of Ainos is regarded as an isolated incident in a general context of a fairly wide Athenian confederacy. No modern has contemplated a date later than Chaeronea, and I think probably the period of renewed war with Philip is also excluded. It isn't an easy point to take if one is reading eighty-odd speeches in a hurry. Whatever excuses you may be inclined to make, Dionysius certainly missed it.

More serious problems are raised by Dionysius' treatment of the two speeches against Boiotos which we know as Dem. XXXIX and XL. Dionysius' treatment here gives us some information from his lost full treatment of Demosthenes, for the first reference, when he is using XXXIX as evidence for another speech, is a reference back to that treatment (§11: 313.20–2); XXXIX, he says, I have shown there to be of 351/0 or 350/49. The reasons for this are restated at §13: 320.3–6. μέμνηται γὰρ ὡς νεωστὶ τῆς εἰς Πύλας ἐξόδου γεγενημένης, ἡ δ' εἰς ⟨Πύλας⟩ ἔξοδος ἐπὶ Θουμήδου ἄρχοντος ἐγένετο, τρισκαιδέκατον ἔτος Δεινάρχου ἔχοντος. Θουμήδου should presumably be Θουδήμου (353/2), but this makes τρισκαιδέκατον wrong and it has always been emended to ὄγδοον. So far only one corruption may be present, though the second error is an odd one. However, our text of the speech does not

[8] Dover, *Lysias and the Corpus Lysiacum* 23.

mention the expedition to Thermopylai but the expedition to Tamynai, and specifically mentions Euboea. Everyone here will be satisfied that this was in spring 348 and that the speech probably belongs to 348/7.[9] I see no escape from the conclusion that Dionysius read the speech carelessly while working on Demosthenes, was confused in mind when coming to his conclusion about the date and did not check it when working on the *Deinarchos*. No help comes from resuscitating τρισκαιδέκατον and emending Πύλας to Ταμύνας, since we would also have to emend the archon, the sentence would still be incorrect and we would have a clash with §11: 313.20–2.

XL raises less acute problems. Dionysius says he showed in the *Demosthenes* that it was two or three years later than XXXIX (§13: 320.6–12). We do not know his reasons, but he could well have been working, as moderns do, from the reference in §37 to Kammys as tyrant of Mytilene. His absolute date will then have been right. His interval from XXXIX was wrong.

So much for the *Deinarchos* and, for the moment, for Dionysius working by himself. For speeches we can check, he does not inspire great confidence.

We now turn to the *Ad Ammaeum*. You will recall that this is directed against an unnamed Peripatetic who had asserted that Demosthenes learnt his rhetoric from Aristotle. The process of refutation goes as follows. We get a life of Demosthenes plus a list of his speeches down to 349/8 (§4: 260.6–262.7), then a complete life of Aristotle (§5: 262.8–263.10), a general case for saying that the *Rhetoric* is late work (§§6–7: 263.11–266.9), and a specific case for dating one passage of the *Rhetoric* after 349/8 (§§8–9: 266.10–268.15). We then return to Demosthenes from 369/8 to 340/39 (§10: 268.16–271.8), then a specific case for dating one passage of the *Rhetoric* after 340/39 (§11: 271.9–275.19), and a final case against anyone who wishes to say that at least the *De Corona* is later than the *Rhetoric* (§12: 275.20–279.8).

The division of the case is of course more rhetorical than scholarly in itself, and the whole thing would have been shorter if he had just produced the latest datable passage of the *Rhetoric*, but I make no quibble with that. I think it is relevant to note the book-bound habit of mind which prevents him from considering any possible way in which Demosthenes could have learnt of Aristotle's views on rhetoric other than consulting a published text

[9] See, e.g., Carey and Reid (eds.), *Demosthenes: Selected Private Speeches* 160: XXXIX late 348, XL before the end of 347. They claim that by 'two or three years' 'Dion. means "a short time" . . . ; he is not being precise.'

of the *Rhetoric*, though he could certainly have known of the view derived by Hermippos from some ἀδέσποτα ὑπομνήματα that Demosthenes had started life as a pupil of Plato καὶ πλεῖστον εἰς τοὺς λόγους ὠφελῆσθαι (Plut. *Dem.* 5.7),[10] which, whatever you think of its historicity, is certainly not irrelevant to Dionysius' problem. Another mental block, which may be even more relevant in the long run, is the failure to recognise that Aristotle's *Rhetoric* could conceivably not be a unitary work, but be composed of strata in such a way that individual datable references might not date the whole work.[11]

Now what are the sources of *Ad Ammaeum*? Here I am fully in agreement with Sealey's treatment as against Schwartz.[12] It is not true, at any rate for this work, that Dionysius is in any way claiming to be dating the speeches of Demosthenes himself by a use of Philochorus or any other historical source. He is certainly using Philochorus for himself here, but he is using him to date passages in the *Rhetoric*. The other information in *Ad Ammaeum* comes from elsewhere: as Sealey says, from standard works of reference. The two main passages are §3: 260.2–4, ἐκ τῶν κοινῶν ἱστοριῶν, ἃς κατέλιπον ἡμῖν οἱ τοὺς βίους τῶν ἀνδρῶν συνταξάμενοι, and §6: 263.11–12, οἱ τὸν βίον τοῦ ἀνδρὸς ἀναγράψαντες. This seems to me, as it did to Sealey, to rule out any suggestion that Dionysius is reporting his own work. In fact, I think it unlikely that the main critical work on Demosthenes to which we have had references in the *Deinarchos* has yet been written. We should however take note of two passages from that work, missed altogether by Sealey, as far as I can see, which do show Dionysius taking the same view, in both cases quite impossible, as the source of *Ad Ammaeum*.

In the first (*Dem.* fr. ii: 290.5–12), he says *Olynthiac II* is the earliest of the *Olynthiacs*, which he justifies by ἄρχοντάς τινας καταλέγων and from the tone of the προοίμιον. In fact, he may well have done here what Schwartz thought he did in *Ad Ammaeum*, that is, used Philochorus to arrange the speeches. Καικίλιος δὲ ἀντιλέγει πρῶτον ἀξιῶν τὸν πρῶτον νομιζόμενον.

[10] This is not quite compatible with the quotation of Hermippos in Gellius III.13 (Demosthenes a pupil of Plato when young, of Callistratos subsequently); cf. also [Plut.] *X Or.* 844B–C (some say Plato, Hegesias of Magnesia (*FGrH* 142F22) says he was captivated by Callistratos).

[11] See most recently G. A. Kennedy, *Aristotle on Rhetoric* 299–305.

[12] Sealey (n. 2, above), 77–80 in general, 81–9 against Schwartz on *Philippic I*. E. Schwartz, *Festschrift T. Mommsen* 1–44, esp. 29–36.

This is, I think, a doubtful case. One could hold that Dionysius was innovating and that Caecilius did talk of τὸν πρῶτον νομιζόμενον. One could also hold that Dionysius was defending a tradition he already had, and that Caecilius was either supporting another tradition or starting a new one, in which case τὸν πρῶτον νομιζόμενον is the scholiast, and not Caecilius. The decision must depend on one's general approach.

I feel more confidence about the second passage, *Dem.* fr. iii: 291.1–5. Here Dionysius is cited as holding the view that a new speech began at IV.30, which is the view in *Ad Ammaeum*. Here he remarks προοίμιον οὐκ ἔχει, ἐπειδὴ δευτερολογία ἐστίν, ἐν αἷς ὡς ἐπὶ τὸ πλεῖστον οὐκ εἰσὶ προοίμια. Antiquity was perfectly capable of detecting that one speech was two. The best example is in Libanius' observation on XXXIV.[13] But this reads to me differently, and I am reasonably convinced that Dionysius is here defending a tradition of a split speech which has already come down to him. I do not think these cases show that *Ad Ammaeum* is dependent on Dionysius' own work, but rather that he was capable of finding arguments in a bad cause. That the *First Philippic* is two speeches is a very bad cause. That the *Second Olynthiac* is the first is a less bad cause, but, if he went on to argue the position of *Ad Ammaeum* that the order is II–III–I, as I am sure he did, it would be quite as bad.[14]

Let us begin with the easier end from which to disentangle Dionysius' sources, the life of Aristotle in §5: 262.8–263.10. Here there is happy unanimity. Comparison with Diog. Laert. V.9 shows remarkable agreement over an enormous span with a named fragment of Apollodorus, and this has been held to settle the matter. Dionysius' text is printed by Jacoby as a parallel fragment with Apollodorus 244F38. There are only minor points for disquiet. Dionysius is after all interested in Aristotle in relation to Demosthenes. In 262.13–14 the point is duly made that Aristotle was three years older than Demosthenes, which doesn't happen to appear in Diogenes, perhaps by accident, perhaps, however, because Apollodorus did not believe it. At the end of the account, the divergence is wider. Apollodorus in Diogenes gives the death-year, synchronous with that of Demosthenes, as he says. Dionysius gives the year, one earlier, of the withdrawal to Chalkis, and, though he concurs on Aristotle's age at death, he does not give the death-year nor the

[13] *Hypothesis* to Dem. XXXIV in O.C.T., §5.

[14] See, e.g., J. R. Ellis, *Hist.* 16 (1967), 108–11 (II–I–III); C. Eucken, *MH* 41 (1984), 193–208 (I–II–III).

synchronism of the deaths. I find it very strange that he should omit the last if he had it before him. There is another discrepancy at the beginning, where Aristotle's ancestry does not appear in the Apollodorus fragment as we have it, but does correspond to the fragment of Hermippos which Diogenes gives in v.1.

I do not want to pontificate about the relation between Hermippos and Apollodorus. The evidence is scanty for Dionysius' use of either so far. We know he used Hermippos for Isaeus;[15] we do not know that he used Apollodorus anywhere else at all. But Hermippos certainly did write a *bios* of Aristotle,[16] which is not a phrase which comes easily for Apollodorus, and I see no *a priori* reason to deny that it contained archon dates. It can only be *a priori* reasoning which allows Jacoby to say, e.g. of Hermippos *ap.* Diog. Laert. III.2, 'Hermippus gehört nur die Todesart' (*ad* 244F37). I do not think we yet know enough of Hermippos to deny him this fairly detailed chronology of Aristotle.

Let us turn back to Demosthenes. Dionysius has at his disposal a life of Demosthenes together with a list of dated speeches. This can be one source or two and does not need to be the same as the source for Aristotle. Even allowing for Apollodorus F14 (Krates) and 43 (Menander), the complexity of the list of speeches certainly rules out Apollodorus as the source for that. We have already seen various places where Dionysius went for *akribeia* – Callimachus and the Pergamene *grammatikoi* (*Dein.* §1: 297.15–16), Demetrios Magnes (*Dein.* §§1–2 *init.*: 298.2–299.10), Hermippos (*Isai.* §1: 93.14). I am inclined to rule out Demetrios as a source for the speech-list, since I think there is nothing to suggest that he gave more than overall totals for anybody. Callimachus and the Pergamenes are a serious possibility, since in *Ad Amm.* 4: 260.17–19 the title of XIV is specifically defined as ἣν ἐπιγράφουσιν οἱ τοὺς ῥητορικοὺς πίνακας περὶ τῶν συμμοριῶν, and no one will now doubt that the whole procedure of defining speeches by their incipits has its roots in pinakography. Perhaps, however, I may be allowed a slight doubt as to whether such a reference does not in fact point away from the main source rather than to it. I would further doubt whether Callimachus and the Pergamenes can ever have been described as οἱ τοὺς βίους τῶν ἀνδρῶν συνταξάμενοι, even if they wrote κοιναὶ ἱστορίαι.

I am in fact arguing that the chances of Hermippos being a main source

[15] *Isaios* §1: 93.12–94.1. [16] Diog. Laert. v.1.

for *Ad Ammaeum* are rather greater than has been generally recognised. I would remind you first that there is a by no means contemptible case for making him responsible for at least the book-list of Theophrastus in Diogenes Laertius,[17] and, even closer to our problem, there are two clear indications that Hermippos concerned himself with the order and dating of Isocrates' speeches. These lie in a quotation from Hermippos in the *hypothesis* to Isocrates II explaining the circumstances of the speech, used in a general context of establishing the order of I and II, and in a clearer one in the *hypothesis* to the *Philippus*, ἔγραψε δὲ ὁ Ἰσοκράτης τὸν λόγον γέρων ὤν, μικρὸν πρὸ τῆς ἑαυτοῦ καὶ Φιλίππου τελευτῆς, ὥς φησιν ὁ Ἕρμιππος. As transmitted, I think we would find this a pretty sloppy statement. Even though the arguments for the spring of 346 are perhaps not all that strong, the review by Speusippos gives us a firmer *terminus ante quem*.[18]

I think that there are other grounds for seeing at any rate conformity between *ad Ammaeum* and Hermippos for the βίος. *Ad Amm.* §4: 260.6–8 firmly states Demosthenes' birth year as 381/0 and goes on to say that he was seventeen in 364/3. A lacuna has presumably removed the reference to the prosecution of the guardians. The chronological statements are of course false. Either they derive from believing that Demosthenes wrote the *Meidias* in 349/8 and working back the thirty-two years of XXI.154 from there, or, as Sealey has shown to be much more probable, they derive directly from a belief that Demosthenes prosecuted his guardians in 364/3 on reaching the age of majority.[19] This belief will have rested on a careless reading of the false reading in MS A of Dem. XXX.15. Sealey could have made his case stronger by pointing to the fact that [Plut.] *X Or.* 844C, 845E, twice gives 364/3 as the date of the prosecution. [Plut.] has available a better view of Demosthenes' birth-year 385/4 (845D) but consistency is not to be expected of him. Comparison of the first page or so of [Plut.] with Plut. *Dem.* 5 leaves no doubt that Hermippos lies behind [Plut.] somewhere. Hermippos is in fact one of the three main sources for Plut. *Demosth.* apart from the speeches of the time, it seems to me. My own inclination is to regard the main narrative as Hermippos on the life, thickened by Theopompus on general history. It is therefore interesting to find Plutarch presupposing the same erroneous birth-year.

[17] Hermippos is cited (not for the book-list) in Diog. Laert. v. 41; the book-list follows in 42–50. See I. Düring, *C&M* 17 (1956), 11–21, esp. 18.

[18] E. Bickermann and J. Sykutris, *Ber. Leipzig* 80 (1928), Heft 3.

[19] *REG* 68 (1955), 99–100.

Ad Ammaeum goes on to say that Demosthenes began to write published speeches at the age of twenty-five, in 355/4. This is peculiar counting, and Schott emended to twenty-seven.[20] Plutarch in 15. 2 gives the first speeches as XXII, XXIV, XXIII, an interesting order, spoken when he was twenty-seven or twenty-eight. *Ad Ammaeum*'s dates are 355/4, 354/3, 353/2. How much more Hermippos there is in Plutarch, we can only guess. One would be interested in the false date 338/7 given for the start of the proceedings against Ktesiphon, based on a false document (XVIII.54; Plut. 24.1–2), the account of relations with Apollodorus and Phormion (15.1), the view that the False Embassy case was never heard (15.3). That Plutarch should have believed that XXI was prepared at the age of thirty-two (12.1) could be an unsurprising conclusion from casual reading, even though it certainly does fit the *Ad Ammaeum* system. There is no trace of Callimachus or the πίνακες being used at all.

The relationship between Hermippos and Callimachus is likely to remain indefinite, and I now propose to go off on another track which may well be held to show that the *pinakes* had done a good deal of the work already.

Dionysius' Demosthenic speeches are all public. Even though the lacuna at §4: 260.8 contained a reference to Demosthenes' guardians, the speeches concerned were not counted in the summation of §4: 262.4. I propose to group the speeches he refers to under four heads: δημηγορίαι, subdivided into Φιλιππικαί and Ἑλληνικαί, a subdivision amply justified by §10: 268.19–20, and δικανικαί, subdivided into speeches made for others and speeches made for himself. For this last sub-division the only evidence in *Ad Ammaeum* is the ὃν αὐτὸς διέθετο of §4: 260.15 applied to the *Leptines*; that it is an ancient distinction is evident from Plut. 15.2. I think the argument will tend to confirm it, for the grouping produces fairly satisfactory results. I am in fact embarked on an argument from order, in face of a warning from Dover:[21] 'The order of individual speeches is never safe ground for argument, because both rational criticism and error played their separate parts. In Harp. 166.3[22] the "eleventh Philippic" of Demosthenes is Dem. X, and XI in fact precedes X in the tenth-century MS Aug. 485.' If I point out that in *Ad*

[20] Reported but not adopted by Usener and Radermacher; not reported by S. Usher (Loeb, 1985); reported and adopted by G. Aujac (Budé, 1992).

[21] *Lysias and the Corpus Lysiacum* 13 n. 12.

[22] Harp. καθήκοντα (166. 3 Dindorf = κ 6 Keaney). Dindorf and Keaney both accept Valesius' emendation of ι΄ for ια΄.

Amm. §10: 270.19 Dem. x is also the eleventh Philippic, because *Ad Ammaeum* splits the *First Philippic*, I hope this may serve to hold Dover supporters off long enough to see where this gets us. Since Aug. 485 (A) has signs of transposition for XI and X, it may never have intended this order.

The results of splitting up *Ad Ammaeum*'s chronological list into these four sections appear in the first column of the Appendix. The order may seem a little strange to us, burdened as we are with the totally unsatisfactory traditional order, which is essentially that of MS F. For comparison, I give three other orders which have better claims to antiquity, that of S, the best text with the largest amount of surviving stichometry, that of A, the wild text from which we have already encountered one very ancient false reading, and that of Libanius. S preserves one group-heading Φιλιππικῶν α'. In fact, as you will see, it can as a whole be divided up without strain into further genre divisions of the type now attested for Lysias by *P. Oxy.* xxxi 2537. I have in fact borrowed from the study of Lysias the heading βίαια to cover my group which consists of one case of αἰκία, three of βλαβή and one of ψευδομαρτυρίαι after βλαβή. βίαια is attested as a group-heading for Lysias, by Harpocration (73. 1 Dindorf = β 12 Keaney) and *Lex. Sabb. s.v.* βιαίων. Such genre divisions are also in part visible in Dionysius' Deinarchos list. This sort of arrangement is also broadly true of MS A, though there are a few more incoherences. In fact, though this may only be an accident of my reading or of reports of MSS, A does have some relevant headings, beginning XIII where I have marked 'Hellenics' Δημοσθένους συμβουλευτικοὶ περὶ συντάξεως and LIV, the first private speech in that MS, ἰδιωτικοί. I was temporarily given cause for wild excitement by Butcher's statement that MS A began with the order IV–I–II–III, which seemed likely to reflect a view that IV was the earliest speech, but though more detailed accounts are slightly discrepant about the battered state of the beginning of A, there seems no ground whatever for asserting that it ever began with IV.[23]

In Libanius most of the groups remain fairly clear, though some of the private speeches are in a slight mess. He follows Dionysius, as he says, in excluding LX and LXI, but not on LVIII. His source, like that of S, seems to

[23] Rhodes has not been able to see A. Spengel, *Ueber die Handschrift Cod. Aug. I Monac. des Demosthenes*. L. Canfora, *Manoscritti* 45, gives the order of the surviving original as IV.28–end, V, VI, etc., with I.II–IV.28 added to the beginning of the MS by a later hand; he also says that I.8–15 and II.16–24 appear at the end of the MS (after the private speeches).

have taken the view that LIX was a public speech, and he is capable apparently of taking a line for himself, for he says of LVIII τοῦτον τὸν λόγον οὐκ οἶδα ὅπως ἐν τοῖς ἰδιωτικοῖς ἀναγράφουσιν οἱ πολλοὶ δημόσιον ὄντα φανερῶς. δῆλον δὲ ἔσται τοῦτο ἐξ αὐτῆς τῆς ὑποθέσεως. It is interesting, I think, that he lists no lost speeches, though there are enough references around to show that our lists are by no means exhaustive.[24]

Some general conclusions seem to follow which do not perhaps concern us much here. I take it that the normal way in which Demosthenes' speeches circulated during the roll era was in these sections, which seem to break down fairly satisfactorily stichometrically.[25] There is clear evidence in S for the Philippics circulating in two rolls, and certainly at least XVIII and XIX will have occupied a roll each. Though the πίνακες had an overall order, there will have been no fixed pattern of arranging the sections when they passed from roll to codex. It seems to me lunatic even to consider whether there is an archetypal codex for the entire corpus. Indeed, the most frequently cited reason for having an archetype at all, the mutilated condition of XXXII, seems to me to prove nothing more necessarily than that neither Alexandria nor Pergamon ever had a complete copy of that particular speech.[26]

The most relevant point which seems to me to emerge is a modification of the implications of a dictum of Dover's:[27] 'It is doubtful whether anyone in ancient and medieval times even attempted so unprofitable an exercise as making a selection from an orator's work on a purely chronological basis.' If this is interpreted to mean that no one ever tried to arrange an edition in chronological order, this now seems to be manifestly untrue for the public speeches of Demosthenes. If we compare the order in *Ad Ammaeum*, as I have broken it down, with that in S, A and Libanius, it is, I think, clear that some chronological ordering has been at work. For the Philippics S has disquieting features but roughly earlier precedes later; in A and Libanius the order is much more rigid. Despite my disappointment over the order of A, I am still inclined to think that the position of IV as we have it may well have been influenced by an early view that the second part of IV was later than the *Olynthiacs*. I can think of no other explanation. For the Hellenics, the

[24] See J. G. Baiter and H. Sauppe, *Oratores Attici* ii. 250–7; R. Clavand, *Démosthène: lettres et fragments* (Budé, 1987), 127–48.

[25] See K. Ohly, *Stichometrische Untersuchungen* esp. 78–80, 101–3.

[26] See, e.g. S. H. Butcher, O.C.T. vol. i, pp. vi–vii.

[27] *Lysias and the Corpus Lysiacum* 3.

order of S and A, XIV–XVI–XV, corresponds with *Ad Ammaeum*. For the court speeches S is particularly convincing and the order XXII–XXIV–XXIII–XX–XXI makes good chronological and logical sense. The scholion which asserts of XXII οὗτος ὁ λόγος μετὰ τοὺς Φιλιππικοὺς πρῶτός ἐστι τῶν δημοσίων[28] is also evidence of some antiquity for an arrangement which, like S and A, separated Philippics and Hellenics and placed XXII at the head of the public court speeches (remember also Plut. *Demosth.* 15.3). Conformity with *Ad Ammaeum* is easy to arrange. For both Hellenics and court speeches our evidence suggests an extension to the chronological order with XVII following XV and XXV–XXVI following XIX–XVIII. All three of the speeches belong to the post-Chaeronea period where Dionysius shortens his case with a final flourish about the *De Corona*. He also happened to disbelieve in their authenticity (*Dem.* §57: 251.1–5). It is easy to see why he should have left them out, and I see no reason to doubt that they were in his source. One particularly distressing fact, I admit, is the constant wrong order XVIII–XIX of all sources other than *Ad Amm.* §10: 270.7–9, §12: 276.12–22. That they had independent rolls is partly the explanation, no doubt, but the only other explanation I can think of, the doubt (Plut. 15.5) as to whether XIX was ever spoken, doesn't seem to help.

The result of all this is to raise the possibility of Callimachus' having done some of the chronological work. For the chances of Hermippos' investigations determining the order of speeches in editions seem to me to be slight, whereas that the *pinakes* did is all too likely. This does not mean that Hermippos did not go over or revise the datings. Clearly he could have done. And it does not mean that Dionysius did not use him rather than Callimachus. The only further point I have to offer on that is that Callimachus called VII περὶ Ἁλοννήσου (*Dem.* §13: 157.6, cf. Lib.) and *Ad Amm.* §10: 270.4–7 does not. Otherwise, I make no further attempt to distinguish between Callimachus and Hermippos.

Of Callimachus' abilities there is little need to speak further. Clearly in his immense task he was liable to serious mistakes, as the whole of antiquity knew, including Dionysius, and down to Photius who emits a testy οὐδ' ἱκανὸς ὢν κρίνειν of him on a point of oratorical authenticity (F446 Pfeiffer). Hermippos may be a less familiar figure.[29] I have given a sample, in what he

[28] 662. 3 Dindorf = 258 no. 1d Dilts (who adopts the alternative reading δεύτερός).

[29] See Drerup, *Demosthenes im Urteile des Altertums* 65–75; Wehrli, *Hermippos der Kallimacheer.*

had to say on Isocrates' *Philippus*, and he was clearly much studied for his accounts of the way people died. Perhaps a selection of his views may be in order. He believed in Jewish influence on Pythagoras (fr. 22 Wehrli), included Triptolemos and Bouzyges among Athenian *nomothetai* (frs. 84, 82). He could name twenty trusty associates of Lycurgus of Sparta (fr. 86). He told a story of Plato buying a book of Philolaos for 40 Alexandrian *mnai* (fr. 40). He thought Polykrates' κατηγορία Σωκράτους was spoken at the trial (fr. 32). He said Aristotle was away in an Athenian embassy to Philip at the time of Plato's death (fr. 45). He said that Thucydides was of Peisistratid descent and that that was why he played down Harmodios and Aristogiton (fr. 62). I admit that this is an unbalanced selection, but it may be as well to know what someone describable by Dionysius as ἀκριβὴς ἐν τοῖς ἄλλοις (*Isaios* §1: 93.14) was capable of.

So much for Dionysius' likely authorities. What of the list itself? What confidence does it inspire? To stick for the moment to what Sealey has established about it, the date for Demosthenes' birth is four years wrong, and the dates of X and XXI are wrong. The list was wrong to divide the *First Philippic* into two, wrong to put the *Olynthiacs* in the order II–III–I. We may add, though Sealey does not, that it includes as speeches of Demosthenes VII and XI although many people in antiquity and all moderns have denied them to him. I said at the beginning we were faced with a question of confidence, and this is what is on record to suggest how great our confidence should be. It is a difficult question to assess precisely, and I wouldn't like to suggest that it should be quite as low as the confidence we should put in the dating of a narrative tract of Diodorus. However, as far as I am concerned, I am quite unable to accept that a date in *Ad Ammaeum* offers any presumption of accuracy, though no doubt some of the dates are in fact right, within qualifications I shall put later.

Not, I hope, much later, but I must first say something about Didymus. First, what light does he throw on the order question? By the time he came to the Philippics, he had already written at least a separate work on the *De Corona*.[30] He was not working through in anything like our Demosthenic order. His third book on the Philippics covered his ninth, tenth, eleventh and twelfth speeches, our IX, X, XI, XIII. We do not know in what order he put I–IV; we do not know whether he had S's eccentric order VIII–VII–VI–V

[30] Cf. Didym. xii.36.

or something more reasonable; we can only reasonably guess that he had covered VII, whatever he said about its authenticity, and did not have the *First Philippic* split. He has one novelty for us, in that he finds himself faced with XIII as a Philippic, whereas S and A clearly classified it as a Hellenic and Libanius notes this is οὐκέτι Φιλιππικὸς ἀλλ' ἁπλῶς συμβουλευτικός. There is of course no mention in it of Philip or Macedon at all, so it is hard to see why ἔνιοι τὸν λόγον εἰς τοὺς Φιλιππικοὺς παρείρουσι. Whoever they were, they are evidently sufficient to determine Didymus' positioning.[31] But though Didymus must, I think, have called it the Twelfth Philippic, no one else does, and it is, for example, the first speech in our corpus to be referred to by Harpocration by an independent name other than Philippic.[32]

Whatever modern scholars may think of the *Ad Ammaeum* list, it is sufficiently clear even from our probably curtailed version, that Didymus was well aware that the whole job needed doing again, and in our text it is the chronology of the speech which is first attended to. It seems that one of the factors impelling him to this was that he was not so fortunate in having a single line to follow. For X, as Lossau has recently argued,[33] he seems to have been faced already with three divergent dates, through which he has to pick his way and about which he argues in some detail, though I do not feel that the details are sufficiently clear for our scrutiny. For XI he says the date is clear, though he doesn't actually say what it is (x.15 sqq.). If [πέρ]υσι in x.51 is right, he has gone over to 339/8, one year later than his evidence necessarily shows and one year later than *Ad Amm.* §10. 271.2–8. He does of course note, as *Ad Ammaeum* does not, that it would not be unreasonable to regard the work as pastiche and that it comes ὀλίγου δεῖν verbatim in Anaximenes (xi.10–14). For XIII he is in considerable trouble, but argues fully (xiii.14 sqq.). It is said to be a Philippic but there is no mention of Philip. It is no use taking the view that it is after 346. I don't feel quite as confident as Lossau[34] that someone has actually already taken the view. The true answer, he says, is 349/8, because he mentions Athenian action against Megara about the sacred *orgas*, a reference to §32, which talks of an Athenian vote to go out

[31] See also the scholion 216.5–6 Dindorf = 163 no. 1, lines 4–6, Dilts.

[32] E.g. *s.v.* Κύθνιοι (186. 1 Dindorf = κ 90 Keaney).
XIII is omitted by Dionysius. Its authenticity is rejected by Sealey, *REG* 80 (1967), 250–5 at 251–3; accepted by Cawkwell, *JHS* 83 (1963), 48 with n. 9 (dating 353/2 after Blass), and championed by Trevett, *GRBS* 35 (1994), 179–93 (dating late 350s).

[33] *Untersuchungen zur antiken Demosthenesexegese* 86–7. [34] *Ibid.* 87.

and stop the accused Megarians who were cutting off the *orgas*. The Athenians took such action in 350/49, as he quotes Philochorus and Androtion to prove. Going down to 349/8 doesn't seem to be more than allowing a year for luck for a back-reference, but apart from that, this is most satisfactory, one would have thought. Here is the fullest argued case we have of a competent ancient critic using evidence, going through the same processes that we infer Dionysius or his predecessors to have done except that we do not have their evidence. It is therefore a little surprising to find that the last scholar who has concerned himself with the speech does not mention that Didymus does give a date, does say that Dionysius does not furnish a date, and proceeds to argue perfectly reasonably that though §32 mentions a vote §33 says no action had followed, and eventually fixes on 353/2.[35] How can it be right, however, to leave a reasoned ancient case on one side to be confuted by implication, while placing faith in unargued statements deriving from a source who certainly had no better brains or information than Didymus?

A few general remarks to end with. I have so far spoken, except in reference to Aristotle's *Rhetoric*, as if a literary work had a single date. I suggest that in the case of Demosthenes this is simply not true.[36] To take the obvious case of XXII and XXIV, and neglecting Dover's attractive arguments that they even have two authors,[37] XXIV will have had a date in the sense that it presumably was delivered in court on a specific day not known to us, but it has passages in it originally written for XXII for a different occasion and at a quite different date.[38] In one sense, the *Meidias* (XXI) has no date at all, for the case was never heard. In another, as Sealey has shown,[39] it has at least two, since there are contradictory time-indications in different passages, which were therefore written at different times. The source of *Ad Amm.* §4: 261.21–262.2 took only one of these points and combined it with a false birth-year to get a 'date'. There are yet other dates, irrecoverable to us, the dates at which

[35] Cawkwell, *JHS* 83 (1963), 47–67 at 48 with n. 9; *REG* 82 (1969), 327–35 at 328–9.

[36] Cf. especially Focke, *Demosthenesstudien*.

[37] *Lysias and the Corpus Lysiacum* 161–3; but see also S. Usher, *GRBS* 17 (1976), 31–40.

[38] Dem. XXII.47–78; Dem. XXIV.160–86.

[39] Sealey (n. 2, above), 96–101. MacDowell, *Demosthenes Against Meidias* 370–1 note on §154, follows H. Erbse, *Hermes* 84 (1956), 135–51, and claims that the figure for Demosthenes' age given there is corrupt and Sealey is wrong; following Erbse likewise, at 10–11 he accepts 347/6 as the date of the trial, and at 23–8 he thinks it possible that the trial took place.

Demosthenes touched the speech for the last time and let it out of his hands for publication, whatever we mean by that.

The importance of these last considerations becomes obvious when we come to consider the problems of VIII, IX, X. These in a sense are in fact four speeches, since IX exists in a long and a short version.[40] In general terms, all the speeches represent the same sort of time and situation, the spring of 341. Philip has been in Thrace for some time, is a threat to the Chersonese and may be represented as likely to attack Byzantion. On the other hand the Euboean situation is still dead against Athens. We are not yet at Skirophorion 341. This is the time when these speeches were *thought*. I do not know when they were spoken. Now we come on to publication. It will be agreed that VIII and IX are in some way more finished than X. It is, I hope, agreed that short IX is a more panhellenic, less Athenian, version of IX.[41] Sealey even thought he could find an argument for dating the cutting-down of IX two years later.[42] One topic, the relations between rich and poor and the theoric fund, only appears in the unpolished X. Possible alliance with Persia appears in X and long IX but not in VIII or short IX.[43] It is clear that we are not going to get any agreed answers, but I hope that it is also clear that we are dealing with three quite different dates, the date when a passage is thought, the date when it is spoken and the date when it is 'published'. These cases are sufficient to show the complexity of Demosthenes' worktable, even if we did not have the *prooimia*, sufficient, I hope, to show that, even if one can date the delivery of a speech, one is not necessarily dating the thinking or writing of any particular passage in it. I fear that it seems to me perfectly respectable to guess, for instance, though I affirm no details here, that large tracts of XXIII were thought and written in 354, were already out of date when the speech was spoken in 353 and that a couple of modernising references to 352 were put in on publication.[44] I see no difficulty whatever in

[40] The short version is in MSS S and L.

[41] On VIII–X see Jackson and Rowe, *Lustrum* 14 (1969), 68–71. The view of IX given here is that of P. Treves, *REA* 42 (1940), 354–64. The currently favoured view of VIII and X, that when VIII was revised for publication the material from X was incorporated in it, was first argued by C. D. Adams, *CP* 33 (1938), 129–44.

[42] *C&M* 23 (1962), 101–10, esp. 103–4.

[43] Rich and poor: X.37–45; theoric fund: X.35–6; Persia: IX.71, X.32–4.

[44] The allusion is to an unpublished paper by P. A. Brunt. At any rate the reference to Phayllus in Dem. XXIII.124 belongs to 352–351; but there are not many references

seeing the *First Philippic* as thought and spoken in 350 with a modernising reference to Olynthus put in on publication in 349.[45] Such additions may be detectable as our knowledge increases. It is the subtractions which worry me. Can we really be sure that the original assembly speeches which lie behind the published *Olynthiacs* contained no slighting references to intervention in Euboea, removed to improve the effect of single-mindedness? It makes a fair amount of difference if that is possible.

Considerations of this kind are unlikely to have been in the mind of any ancient scholar, and this is the final reason why we are not dispensed from using our own judgement. I do not deny that it makes life very hard, but I cannot do anything about that.

to events later than 357–356, and, for instance, §103 belongs to a time when there were still three kingdoms in Thrace.

[45] E.g. Jaeger, *Demosthenes* 121–2 (but it is now generally believed that the reference in §17 is to an earlier Macedonian incursion into Olynthian territory). On the date of the *First Philippic* see Jackson and Rowe, *Lustrum* 14 (1969), 59–61; Cawkwell, *CQ* n.s. 12 (1962), 122–40 at 122–7 = Perlman (ed.), *Philip and Athens* 48–66 at 48–53; J. R. Ellis, *REG* 79 (1966), 636–9; (Hammond and) Griffith, *History of Macedonia* ii.296–7.

APPENDIX
Arrangements of Demosthenes' speeches

Dion. Hal. *Ad Ammaeum*	**Demosthenes** MS S	**Demosthenes** MS A	**Libanius**
Philippics	Philippics I	Philippics	Philippics
IV A	I	I	I
II	II	II	II
III	III	III	III
I	IV	IV	IV
IV B	VIII	V	V
V	VII	VI	VI
VI		VII	VII
VII	Philippics II	VIII	VIII
VIII	V	IX	IX
IX	VI	X	X
X	IX	XI	XI
XI	X		
	XI	Public Court (confused)	Hellenics
Hellenics		XXII	XIII
XIV	Public Court (others)	XXI	XIV
XVI	XXII	XXIII	XV
XV	XXIV	XVIII	XVI
	XXIII	XIX	XVII
Court (others)		XXIV	
XXII	Public Court (self)	XX	Public Court (mixed)
XXIV	XX	XXV	XVIII
XXIII	XXI	XXVI	XIX
	XVIII		XX
Court (self)	XIX	*Biaia*	XXI
XX	XXV	LIV	XXXIII
XXI	XXVI	LV	XXII
XIX		XLVIII	XXIV
XVIII	Public Court (others)	LVI	XXV
	LIX		XXVI
		Epitropikoi	LIX
	Paragraphai	XXVII	LVIII
	XXXVI	XXVIII	LVII
	XLV–XLVI (tied to XXXVI by subject)	XXIX	
		XXX	*Epitropikoi*
		XXXI	XXVII

Demosthenes MS S	**Demosthenes** MS A	**Libanius**
XXXVII	*Paragraphai*	XXVIII
XXXVIII	XXXVII	XXIX
XXXII	XXXV	XXX
XXXIII	XXXVIII	XXXI
XXXIV	XXXIV	
XXXV	XXXII	Mixed
	XXXVI	LIV
Prooimia	XXXIII	XXXIX
		XL
Letters	Hellenics	
I	XIII	*Paragraphai*
IV	XIV	XXXVI
II	XVI	XLV–XLVI
V	XV	(tied to XXXVI
III	XVII	by subject)
		XXXII
Epitropikoi	Odd	XXXVII
XXVII	LI first time	XXXVIII
XXVIII		XXXV
XXIX	Property	XXXIV
XXX	XLIII	XXXIII
XXXI	XLIV	
	XXXIX	Odd
Biaia	XL	LV
LIV	XLI	
LVI		Apollodorus
XLVIII	Apollodorus	LII
XLVII	XLIX	LI
LV	LIII	L
		XLIX
Apollodorus	Odd	LIII
L	XLII	
LI		Mixed
LIII	Apollodorus	XLII
XLIX	L	XLI
LII	LI second time	XLVIII
		LVI
Property and	*Ephesis* and	XLVII
Inheritance	*Endeixis*	XLIII
XXXIX	LVII	XLIV
XL	LVIII	
XLI		Missing
XLII	Odd	XII
XLIII	XLVII (frag.)	
XLIV		

Demosthenes MS S	**Demosthenes** MS A	**Libanius**
Ephesis and	Missing	LX–LXI
Endeixis	XII	(see *Vita* 20)
LVII	XLV	*Prooimia*
LVIII	XLVI	Letters
	XLVII (most)	
Epideictic	LII	
LXI	LIX	
LX	LX	
	LXI	
Hellenics	*Prooimia*	
XIII	Letters	
XIV		
XVI		
XV		
XVII		
Missing		
XII		

27

Law on the Lesser Panathenaia

Fragment of Pentelic marble, preserving part of a double moulding at the top, brought in from the vicinity of Evangelistria Street (Judeich, *Topographie von Athen*[2], Plan 1, squares G3–4) on 27 May 1938.

Height, 0.326 m; width, 0.37 m; thickness, 0.111 m.
Height of letters, 0.005 m, in a square chequer pattern of 0.0103 m.
Inv. No. I 5477.

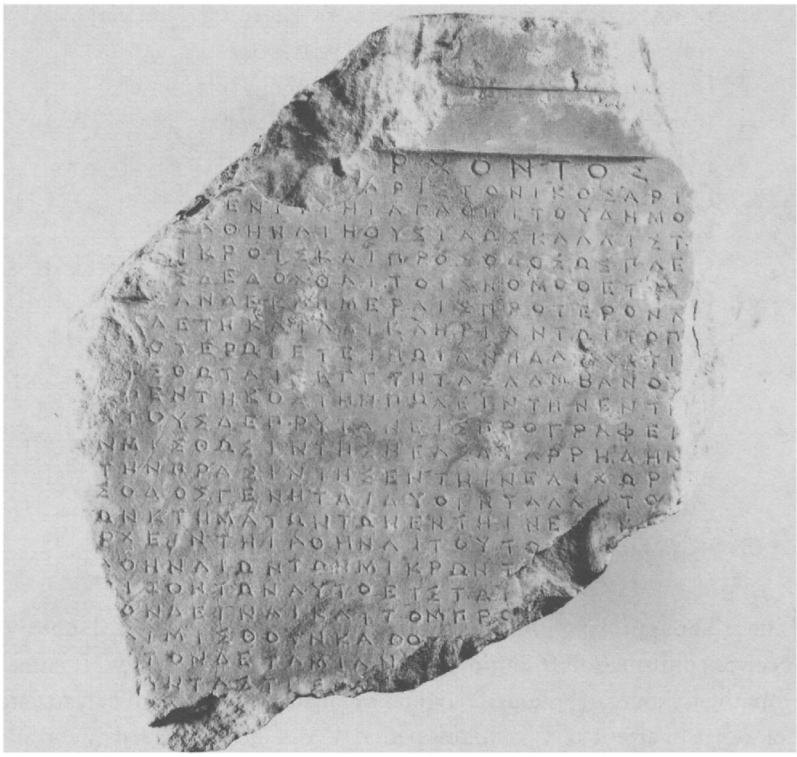
See Plate 4.

Content, lettering and spacing all make it certain that we have here the top of *IG* ii[2] 334 (E. M. 7153). It will be seen that it contains a law relating to the financing of the Lesser Panathenaia. The old fragment contains a decree of the *demos* about the organisation of the festival, apparently in amendment of a *probouleuma* of the *boule*, which also must have stood on the stone, since lines 16–17 of the old fragment presuppose information which cannot have stood in our law. This combination of a law and a decree on the same stele is unparalleled, but is justified by the permanent nature of the provisions of the decree.[1]

Unfortunately, although the length of the lines is certain, the horizontal position of the new fragment cannot be precisely fixed, since it has no edge to left or right. Broken surface extends to the right sufficiently to make it clear that there were at least five letters to the right of the last preserved letter, and I assume, for reasons which will appear, that there cannot have been many more.

* Published in *Hesperia* 28 (1959), 239–47; cf. *SEG* xviii 13. L. Robert, *Hellenica* 11–12 (1960), 189–203, suggested that the Nea is Oropus, recovered by Athens through Philip's settlement after Chaeronea, in which case the *pentekoste* in line 12 can as usual be an import and export tax. For a recent edition of the inscription see Schwenk, *Athens in the Age of Alexander* no. 17.

[1] My thanks are due to B. D. Meritt for entrusting me with the publication, to E. Vanderpool for help with readings, and to M. I. Finley and A. M. Woodward for help and suggestions.



4 Law on the Lesser Panathenaia: *SEG* xviii 13.

ΣΤ. 42

[Θ ε] ο [ί]
[Ἐπὶ . . . . c. 9 . . . . . ἄ]ρχοντος
[. . . . . 16 . . . . .]ῳ[. . 4 . .]. Ἀριστόνικος Ἀρι[στοτέλο]
[υς Μαραθώνιος εἶπ]εν· τύχηι ἀγαθῆι τοῦ δήμου [τοῦ Ἀθη]
5 [ναίων, ὅπως ἂν τῆι] Ἀθηνᾶι ἡ θυσία ὡς καλλίστ[η ἥι Πανα]
[θηναίοις τοῖς μ]ικροῖς καὶ πρόσοδος ὡς πλεί[στη γέν]
[ηται ἱεροποιο]ῖς, δεδόχθαι τοῖς νομοθέτ[α]ι[ς· τὴν μὲν]
[Νέαν μισθούτω]σαν δέκα ἡμέραις πρότερον γ[. . . 7 . . . .]
[οἱ πωληταὶ δέκ]⟨α⟩ ἔτη κατὰ δικληρίαν τῶι τὸ π[λεῖστον]
10 [διδόντι τῶι πρ]οτέρωι ἔτει ἢ ὧι ἂν ἡ ΔΑ.ΑΝΤΙ [. . . 7 . . . .] ||
[. . . . . 10 . . . . . μι]σθωταῖς ἐγγυητὰς λαμβάνου[σι. τοὺς δ]

[ἐ πωλητὰς τὴ]ν πεντηκοστὴν πωλεῖν τὴν ἐν τῆ[ι Νέαι χω]
[ρὶς τῶν ἄλλω]ν. τοὺς δὲ πρυτάνεις προγράφει[ν βουλῆς]
[ἕδραν εἰς τὴ]ν μίσθωσιν τῆς Νέας διαρρήδην [καὶ τῆς π]
[εντηκοστῆς] τὴν πρᾶσιν τῆς ἐν τῆι Νέαι χωρὶ[ς . . . 6 . . .]
[. . . 6 . . . ἡ πρό]σοδος γένηται δυοῖν ταλάντο[ιν . . . 6 . . .]
[. . . . 9 . . . . . τ]ῶν κτημάτων τῶν ἐν τῆι Νέα[ι] κ[. . . . 9 . . . . .]
[. . . . . 10 . . . . .]ρχειν τῆι Ἀθηνᾶι. τοῦτο [. . . . . 13 . . . . .]
[. . . 7 . . . . Παν]αθηναίων τῶν μικρῶν τ[. . . . 9 . . . . . οἱ δὲ ἀ]
[ποδέκται με]ριζόντων αὐτὸ εἰς ταῦ[τα τοῖς ἱεροποιο]
[ῖς . . . . 9 . . . . .]ον δ' εἶναι καὶ τὸμ προ[. . . . . 14 . . . . .]
[. . . . . 11 . . . . .]αι μισθοῦν καθὸ [. . . . . 16 . . . . .]
[. . . . . 11 . . . . .]ι. τὸν δὲ ταμίαν [. . . . . 19 . . . . .]
[. . . . . 12 . . . . .]ονταστησ.[. . . . . 20 . . . . .]
[. . . . . 13 . . . . .]^[----------]

Commentary

Line 2: The approximate date of the inscription has never been in doubt and receives confirmation from the name of the proposer of the law. It comes from the Lykourgan period of religious organisation, and is unlikely to date before 336 or after 330. Within this period we can only be guided by considerations of spacing, and those so uncertain that I abstain from reproducing my calculations. I am inclined to say that, even if we restore as little as possible to the right of the fragment, that is, one letter less than I have allowed in my text, the shortest possible restoration [ἐπὶ Νικήτου ἄ]ρχοντος (332/1) will still be a little too long to sit symmetrically in the centre of the stele. If the line started fairly near the left-hand edge, still on the assumption that we have as little as possible to the right, ἐπὶ Κτησικλέους (334/3) and ἐπὶ Νικοκράτους (333/2) will be a little too long, ἐπὶ Πυθοδήλου (336/5) and ἐπὶ Εὐαινέτου (335/4) a little too short. As the right-hand margin is moved out, 336/5 and 335/4 become possible, as do the even shorter archons of 337/6 and 332/1. The archons of 331/0 and 330/29 are too long to come into consideration. The year 332/1 has the attraction that during it similar provision for the financing of the Amphiareia was worked out by the atthidographer Phanodemos (*SIG*^{3} 287, lines 10–16), but special conditions applied in Oropos and I am inclined to follow all previous editors and place this law rather earlier. The spacing I have adopted here allows the restoration of the archons of either 336/5 or 335/4.

Line 3: Vanderpool and I are fairly confident that the letter in the seventeenth space is *sigma* of the straight type found in this inscription. The horizontal bottom stroke is clear, and there appears to be the start of a diagonal leaving its left end. || Neither of us would exclude the possibility of an unusually small *delta*, but consider it much less likely epigraphically. *Sigma*, however, cannot be fitted into any formula we have in the prescript of an Athenian law (cf. *IG* ii² 140, 244, 333; *Hesperia* 21 (1952), no. 5; Demosthenes, XXIV.71). But the five prescripts we possess exhibit such diversity that we cannot claim to have an exhaustive list of possibilities, and, although *delta* will allow either a calendar date, e.g. [Σκιροφοριῶνος ἐβ]δ[όμηι], or an abbreviated prytany-indication [ἐπὶ τῆς . . . ίδος ἐβ]δ[όμης], I have thought it best to put in the text the most likely *reading*.

Lines 3–4: Ἀριστόνικος Ἀρι[στοτέλους Μαραθώνιος]. About the demotic, there can be no doubt. This is *PA* 2028, the well-known politician of this period, known as a colleague of Lykourgos in 335/4 from *IG* ii² 1623, lines 280–2. It has always been likely, and I consider it certain, that he is to be identified with the Aristonikos of Alexis, frags. 125–6 (Kock), who is a noted νομοθέτης, who, according to Alexis at any rate, is turning his attention to the fish-trade. I quote the most relevant passages:

οὐ γέγονε κρείττων νομοθέτης τοῦ πλουσίου
Ἀριστονίκου . . . . . .
οὐ γέγονε μετὰ Σόλωνα κρείττων οὐδὲ εἷς
Ἀριστονίκου νομοθέτης· τά τ' ἄλλα γὰρ
νενομοθέτηκε πολλὰ καὶ παντοῖα δή,
νυνί τε καινὸν εἰσφέρει νόμον τινά.

It is satisfying to find a real law proposed by Aristonikos. Like Eukrates, the proposer of the first law published from these excavations (*Hesperia* 21 (1952), no. 5), he came to an unpleasant end, and he is linked with him by Lucian (*Demosthenis Encomium* 31).

For the patronymic, I have adopted a suggestion made to me by A. M. Woodward. Despite the size of the deme, political families from Marathon are rare, and it is extremely tempting to see the father of Aristonikos as Aristoteles (*PA* 2065), proposer of *IG* ii² 43, the foundation-charter of the Athenian confederacy, and active precisely a generation earlier. There are, of course, other names in Ἀριστο- which fit equally well, but the probability that a rich colleague of Lykourgos will be found to have a prominent father is considerable enough to justify the admission of the conjecture to the text.

Line 4: τύχηι ἀγαθῆι. The reverse order is more usual (*Hesperia* 21 (1952), no. 5, line 5; *IG* ii² 244, line 2), but there are ample parallels, even one from Aristonikos' putative father (*IG* ii² 43, line 7).

Line 5: I was at first inclined to read another letter, ὡς κάλλιστα [ἦι], and there is of course ample evidence for the construction, but the trace seems to lie too || far to the left, and I think I have been misled by a scratch. I therefore prefer καλλίστ[η] (cf. *SIG*³ 287, lines 13–14: ὅπως ἂν ἥ τε πεντετηρὶς ὡς καλλίστη γίγνηται).

Line 6: [Παναθηναίοις τοῖς μ]ικροῖς and compare line 19. This is a surprising variation in terminology, since the old fragment (line 32) uses the term τὰ Παναθήναια τὰ κατ' ἐνιαυτόν. It is clear that Deubner (*Attische Feste*, 23) was wrong to conclude from this and from Harpokration *s.v.* that τὰ κατ' ἐνιαυτόν was the official terminology, against Lysias XXI.2 and Menander frag. 428 (Koerte), which use μικρά. If the *nomothetai* use one form and the *demos* another, there can be no official terminology.

Lines 6–7: For the thought, compare again *SIG*³ 287, lines 13–16. Line 16, as well as the whole subject matter, makes it clear that πρόσοδος here means 'revenue'. This makes [τοῖς ἱερο]ῖς a much less likely restoration than [ἱεροποιο]ῖς. For these *hieropoioi*, see the old fragment, line 6, with Kirchner's note, which rests on Ziehen, *RM* 51 (1896), 212.

Lines 7–11: I take it that these lines order the leasing of the Νέα, for which a special meeting of the *boule* is ordered in lines 13–14. If this is so, it is most likely that the reference to the Νέα will stand first in the sentence, and I do not see that]σαν is likely to be anything but the end of the late form of the third person plural imperative (cf. *IG* ii² 204, line 47, of 352/1, for a fourth-century epigraphic use of this form). There is of course no reason to try to find room for the eccentric mixed form μισθούντωσαν of *IG* ii² 1241, line 52.

What is the Νέα? In Theophrastos, *De Causis Plantarum*, III.20.7, the sense is clearly the same as in the cognate νειός or νεός, land which is being rested from cereals and planted with some kind of pulse, as a modification from the ordinary two-year cycle. The word comes twice in leases. In one, *SIG*³ 963, lines 45–6 (Arkesine), it seems that the ground contains an area of νέα already, which the tenant is required to dig up, for line 8, though the reading and interpretation are uncertain, seems to make the use of this method in the future optional. In *IG* ii² 2493, lines 7–9, the situation is rather different, since the tenant is to be compelled to keep a quarter of his ground in this condition, leaving a quarter completely fallow, and there is a near

parallel in *IG* ii² 1241, lines 21–3, where the word does not appear, but where the tenant has always to leave half the land uncultivated for cereals, but can use his judgment about how much he will sow with pulse. Having said so much, I hope it is now clear that we must be dealing with something rather different here and that Liddell-Scott-Jones is wrong to include *IG* ii² 334, line 17 as an example of the basic use of the word, since no one could have been expected to pay a large rent for ground which could only be sown with pulse. I therefore assume that the Nea must be a specific and well-known area of state-owned land, which may have been put under this form of cultivation to restore it, and I have given it a capital letter. It must have been fairly considerable in size, since we learn from line 17 of the old fragment || that its leasing fetched 41 mnai. No Attic rent is anywhere near as large as this; the only comparable figure is 7,110 dr. for all the sacred land on Rheneia in 432 (Tod 54, line 24) and perhaps we should not be too ready to assume from its name that its value was only for cultivation.

A solution for the end of line 8, which will provide us with the exact date required, should be easy to find, but I have failed to find a satisfactory one, either with the *nu*, which seems to me the more likely reading, or with *mu*, which, though malformed, would be possible. I offer as a remote possibility Ν̣[εμεσίων] (cf. Pouilloux, *Forteresse de Rhamnonte*, no. 15, line 28). The Nea would then presumably be in the neighbourhood of Rhamnous.

Lines 9–10: [δέκ]⟨α⟩ ἔτη. It is with some reluctance that I assume a mistake in cutting (for the first letter of line 9 certainly has no crossbar) in order to support a restoration. But I can make nothing of ΛΕΤΗ; a term of years is certainly required, and ten years is the duration of state leases of *temene* (Cf. *Ath. Pol.* 47. 4).

κατὰ δικληρίαν. This is the first instance of δικληρία in Greek. I do not see that it can mean anything but 'in two sections', and this seems to be confirmed by the appearance of μισθωταί in the plural in line 11.

τῶι τὸ π[λεῖστον διδόντι]. Cf. *IG* ii² 2492, line 36.

The end of line 10 is completely bewildering, but Vanderpool and I are in complete agreement on the readings and regard even the *nu* as nearly certain. Various suggestions, presupposing greater or lesser mistakes in cutting, have been made in conversation or correspondence, but none of them gives any kind of sense. The two most promising, ἡ ἀλ̣[λ]ακ̣τι[κὴ] and ἡ (ἀ)δα[μ]αντί[νη] will suffice to show how far we are from an interpretation. A subjunctive like γένηται will be required to complete the subordinate

clause,[2] and the amount of space available makes it unlikely that [μι]σθωταῖς ἐγγυητὰς λαμβάνου[σι] is anything but a curiously curt phrase in apposition to τῶι τὸ π[λεῖστον διδόντι]. For λαμβάνειν ἐγγυητήν cf. Demosthenes, XXXIII.7, which is not an exact parallel, since there it is the creditor who is the subject of λαμβάνειν. We would expect rather some form of καθίστημι, but I can think of no way of turning the sentence.

Lines 11–13: Taken together with line 15, these lines must certainly indicate that the πεντηκοστή on the Nea is to be farmed. I have no wish to embark here on a re-examination of the Athenian taxation-system, and therefore curtail my references to modern literature, but this information suggests that such a re-examination is certainly desirable. The general view, held, for example, by Boeckh, Francotte, Busolt-Swoboda and Andreades, which brings all references to a πεντηκοστή in Attica under the single heading of a 2 per cent import and export tax, as described in various forms by the lexicographers, and as seen in operation in Demosthenes, XXXIV.7, XXXV.|| 29–30, will certainly not cover this case. Nor is it certain that all the other references should be so interpreted. The πεντηκοστή farmed first by Agyrrhios and then by Andokides around 400 (Andokides, I.133), may or may not have been such a tax. The πεντηκοστὴ τοῦ σίτου of 368 (Demosthenes, LIX.27), which involved a separate farming-operation of its own, may have been a tax on imported corn or it may have been a produce-tax on home-grown corn. We are too easily inclined to think that a produce-tax disappeared from Attica with the Peisistratids. Such a tax may be the explanation of the mysterious δεκάτη of the first Kallias Decree (*ATL* ii. D 1 = *IG* i^3 52. A, line 7). Other passages which may have to be borne in mind include the neglected Demosthenes, XXIV.120, which certainly seems to imply that some people were farming τὰς δεκάτας τῆς θεοῦ καὶ τὰς πεντηκοστὰς τῶν ἄλλων θεῶν, and *IG* ii^2 404, line 16 (a produce-tax extended to Keos?). All that can be said now of the passage before us is that the πεντηκοστή on the Nea is not an import-tax, and is quite possibly a produce-tax. It seems also that this tax covers a wider area than the Nea, but that the tax on the Nea is to be farmed separately, in order to make it easier to earmark it for the Panathenaia, but, in default of a satisfactory restoration for the end of line 15, my restoration here [χωρὶς τῶν ἄλλω]ν (*sc.* πεντηκοστῶν) is by no means certain. To farm a tax to be collected from two people is odd

[2] Woodward suggests that another estate is named here and the subjunctive is μισθωθῆι.

procedure, but need imply no more than demanding an itemised bid from the general farmers.

Line 13: προγράφει[ν βουλῆς ἕδραν] I have not yet found a parallel for the complete phrase, but in *IG* ii² 244 it is a certain restoration in lines 36–7 and a likely one in line 9. Something like it seems to be required here, cf. *Ath. Pol.* 47.2. Perhaps, alternatively, προγράφει[ν ἐν τῆι β|ουλῆι καὶ τὴ]ν μίσθωσιν (Woodward).

Line 14: διαρρήδην. The restoration here may arouse legitimate doubts. It seems to give διαρρήδην a sense much nearer 'exclusively' than its normal 'explicitly', but, when *IG* ii² 244, line 9, wishes to say this, it has αὐτὸ καθ' αὑτο. What it ought to mean is that the *prytaneis* shall give this operation a special item on the agenda, instead of including it among other μισθώσεις, but the parallels (Lysias, xxxi.27; Demosthenes, xx.133) for the word standing on its own and not in close association with a word of stating, witnessing, allowing or forbidding are hardly close. But the alternative, to suppose that we should imagine a comma before the word and take it with a participle in line 15 beginning with χωρι[has not yet for me yielded any restoration which fits or makes sense.

Lines 16ff.: My original approach was to suppose that line 16 fixes a reserve-price below which the leasing of the land and the farming of the two-per-cent tax should not be sold. But since we learn from lines 16–17 of the old fragment that the Nea was in the event leased for only 4,100 drachmai, this makes Aristonikos absurdly unrealistic. Woodward would meet this difficulty by supposing that the other estate he hypothesises for line 10 was referred to also in lines 17–18. Our combined suggestions for lines 15–18 would then run: ||

χωρι[σθεῖσαν],
[ὅπως ἂν ἡ πρό]σοδος γένηται δυοῖν ταλάντο[ιν κατὰ ἐν]
[ιαυτὸν ἀπὸ τ]ῶν κτημάτων τῶν ἐν τῆι Νέα[ι] κ[αὶ τῆς . . . .]
[. . . ὥστε ὑπά]ρχειν τῆι Ἀθηνᾶι τοῦτο [τὸ ἀργύριον . . .]

It is however doubtful if this version really provides enough space for the second estate, nor is it easy to find a satisfactory continuation. Finley points out that there is no satisfactory parallel for fixing a reserve-price in these terms, and would prefer an alternative approach, providing for the contingency that too much money would come in from the lease. Something along these lines might then be possible:

χωρὶ[ς τῶν ἄλλ]
[ων. ἐὰν δὲ πρό]σοδος γένηται δυοῖν ταλάντο[ιν κατὰ ἐν]
[ιαυτὸν ἀπὸ τ]ῶν κτημάτων τῶν ἐν τῆι Νέα[ι] κ[αὶ τῆς πεντ]
[ηκοστῆς, ὑπά]ρχειν τῆι Ἀθηνᾶι.

But unless some phrase expressing a surplus can be found to replace κ[αὶ τῆς πεντ|ηκοστῆς] the result is curt and obscure, though the distinction between two funds, that of the *hieropoioi* and Athena's own, might be clear enough.

My feeling is that there ought to be a full stop after Ἀθηνᾶι. I have considered the possibility of continuing the sentence τοῦ τό[που], but this is hardly an official word, and is used of a district in Attica only in the rather colloquial passage, Demosthenes, XXI.158. τοῦ τό[κου] is of course out of place here. The trouble with τοῦτο is that both it and αὐτό in line 20 suggest that some word like ἀργύριον has come before, and I can think of no way of introducing it into lines 17–18 without involving the absurdity mentioned above. τοῦτο [δὲ καταβάλλειν | αἰεὶ πρὸ Παν]αθηναίων τῶν μικρῶν τ[--] would be satisfactory enough for lines 18–19, if the lengthened form αἰεί be admitted as late as this. The sentence will be completed either with those making the payment or with those to whom they pay it. In any case, some reference to the *apodektai* is indispensable in the gap of lines 19–20, for the first letter of line 20 is a nearly certain *rho*, which implies [με]ριζόντων, and it would be perverse to separate them from their normal function.

ταῦτα in line 20 presumably refers to the Panathenaia. The rest seems quite hopeless. In line 22, it seems difficult to see anything but what would be, I think, the first appearance of καθὸ in epigraphic Attic. In lines 23–4 there seems to be a reference to the ταμίας τῶν στρατιωτικῶν, who has functions connected with leases (*Ath. Pol.* 47.2), but it does not seem possible to resolve line 24 with any certainty.

I hope that the difficulties in this document will receive attention from others and that more satisfactory solutions will be reached. It seems to me a document of considerable importance. Our knowledge of Athenian financial procedure as laid down || by νόμοι is still extremely slight. Demosthenes, XXIV.96–8 and *Ath. Pol.* 48 give us some foundation for an account of the law on the μερισμός, and the need for a νόμος to establish new and permanent financial obligations has long been known from such passages as *IG*

ii² 222, lines 41–6, *SIG*³ 298, lines 39–45.[3] What is new here, besides the fact of actually having an instance of the *nomothetai* at work on financial matters, is the earmarking of specific sources of revenue for a specific purpose.[4] Even at a lower level than that of the state, *IG* ii² 1172 is not an exact parallel, for there the deme is merely making sure that it has a regular income, out of which it hopes to fulfil all its religious obligations. The concept of making sure that there are specific funds for a specific purpose seems quite new in Athenian public finance. The need to do this in this case seems to arise from the date of the Panathenaia, towards the end of the first prytany of the year. Admittedly, the biggest inflow of Athenian revenue came in the ninth prytany (*Ath. Pol.* 47.4), but this was probably followed by the biggest out-payments. At the end of the tenth prytany, officials would have to be paid off and deficits might have to be met on certain funds. At the beginning of the first prytany, floats would have to be provided for the new boards. The Panathenaia would fall a little later than these big demands on the revenue, and in a bad year there might not be enough to meet the proper demands of the festival. I suspect that we find this happening as early as the last years of the Peloponnesian War. In 415 the *tamiai* hand over 9 talents to the *hellenotamiai* for the *athlothetai* of the Lesser Panathenaia.[5] The payment is indicated by ἐδανείσα[μεν], not by παρέδομεν, and, despite the doubts of Meyer,[6] this ought to indicate a difference in the circumstances of the payment. That difference consists, I suggest, in the lack of precedent for the payment. The Panathenaia would normally be financed out of ordinary revenue, but in this case the ordinary revenue was not in a position to meet what may have been an extraordinary demand. The payment was made out of what was technically the wrong fund, and ἐδανείσαμεν, I think, expresses this.[7] Similar payments follow in 410 (*IG* i² 304, lines 5–6) and 405 (*IG* i² 305, lines 8–9).

[3] See, most recently, for the whole topic, Jones, *Athenian Democracy* 102–3.

[4] But compare Isokrates, *Areopagiticus* 27, where he seems to complain that, while lavish state-aid is given to ἐπίθετοι ἑορταί, some traditional sacrifices are forced to depend on μισθώματα.

[5] *IG* i² 302, lines 56–8 = i³ 370, lines 66–8. The amount seems large, and it may have been thought that a lavish celebration was in order after the departure of the great fleet and the political troubles of the year. For an alternative view of this and the later payments, see Davison, *JHS* 78 (1958), 32–3.

[6] *Forschungen zur alten Geschichte* ii. 135 note 1.

[7] Cf. the later similar use of προδανείζω, *SIG*³ 298, line 39, *IG* ii² 330, line 62.

But the possibility of such a crisis is most clearly documented in the fourth century by Demosthenes, XXIV.26–9. There is some uncertainty as to whether we are dealing with the Lesser or the Greater Panathenaia here, since, although Dionysios of Halikarnassos (*Ad Ammaeum*, I.4) dates the speech to 353/2, there is a case for || 354/3,[8] which would imply the Greater Panathenaia. Whatever the truth is about this, the meeting of the *nomothetai* which gave rise to the speech was originally occasioned by an alleged crisis, discovered seventeen days before the festival, about its financing. The *nomothetai* were summoned ὅπως ἂν τὰ ἱερὰ θύηται καὶ ἡ διοίκησις ἱκανὴ γένηται καὶ εἴ τινος ἐνδεῖ πρὸς τὰ Παναθήναια διοικηθῇ. Demosthenes alleges that there was in fact no crisis and says that no one proposed any law to meet the crisis. The second statement must be true, and it is difficult to think of any law which would have met such a crisis in time for that year's festival. But it is hard to think of the crisis as anything but a fact; it must have at least seemed plausible that the διοίκησις would not be able to meet the demands of the festival. Timokrates could have related his law to the crisis by claiming that he was making sure that adequate revenues to carry the διοίκησις over its crisis period did at least come in during the ninth prytany. Demosthenes stands the solution on its head. and complains (paragraphs 98–9) that the result will be that no money will come in until the ninth prytany and that there will be a shortage of money during the rest of the year.

Twenty years later the possibilities of such a crisis are still before Aristonikos, and, to protect the Panathenaia, he devises the solution of earmarking specific revenues for it. It seems likely that there was a separate law for the Greater Panathenaia, and that other income besides the revenue from the Nea was provided for it. Our stone continued with general regulations for the conduct of the festival, laid down by normal assembly procedure, after the result of the leasing of the Nea was known. I have little to add on *IG* ii^2 334. There is a later text in *SIG*^{3} 271, with a small correction in line 31 and a restoration contrary to the traces on the stone in line 30. The standard discussion is now Deubner, *Attische Feste*, 24–6. There is an important note on the disputed restoration in line 10 by Herington, *Athena Parthenos and Athena Polias* 31.

[8] *BSA* 49 (1954), 32.

28

The Athenian Rationes Centesimarum

Even in Athens, our understanding of matters to do with landholding is imperfect. I discuss here a group of texts which raise problems which are interesting and intractable.[1]

The number of fragments of stone with which we are concerned is fifteen, which have gradually accumulated. They have been shamefully neglected; their bibliography is short, and, with some major exceptions, unhelpful. Their basic pattern is clear. They name sellers, the designation of the real property sold, the purchasers and the price they paid. When the sellers only sell one property, the note ἑκατοστή followed by 1 per cent of the price, exactly calculated, follows. When they sell more than one, their entry ends with a total and then the ἑκατοστή on that. In two places only, totals appear which evidently cover a group of individual sellers. These too are followed by the ἑκατοστή.

One such fragment was known to Boeckh,[2] from a slightly inaccurate copy. Apart from some unlucky tinkering with badly read figures, his contribution was to make the association of the text with the fragment of Theophrastus[3] which says that at Athens you have to register a sale of property with the magistrate not less than sixty days before completion and that the purchaser has to put down a ἑκατοστή of the price so that there shall be an opportunity for anyone to dispute the transaction and that the legal purchaser may be clear by reason of his payment. I do not wish to discuss this fragment, though it would be pleasant to be sure whether it is the seller or

* Published in Finley (ed.), *Problèmes de la terre en Grèce ancienne* (1973) 187–212 (Mouton). V. J. Rosivach, *Eirene* 28 (1992), 49–61, argues that the transactions should if possible be interpreted as leases rather than sales. A detailed study of the texts by S. J. Lambert is awaited.

[1] I am grateful to M. I. Finley and V. N. Andrejev for reading an earlier version of this paper. The extent of their disagreement should be amply clear from the text.

[2] *Staatshaushaltung der Athener*[3] ii. 310–12. [3] Fr. 97 Wimmer.

the state who gets the ἑκατοστή, and I think its introduction into the study of these texts has been unfortunate in that it has created the impression that we are dealing with routine transactions. ||

Many more fragments were known to Koehler, who made the fundamental observation[4] that all sellers in these texts were corporations and that no private sellers appeared. The number of texts has roughly doubled since he saw this, without counter-evidence. His only other contribution, besides careful reading of the texts, was to introduce what is now *IG* ii² 1471.10–13, gold phialai made from a ἑκατοστή by the Treasurers of Athena in 321/0. The implication would be, as Lipsius saw,[5] that the state kept the ἑκατοστή, but I cannot believe that this text has anything to do with ours. The practice of the Treasurers of the period makes it reasonably certain that, whatever the ἑκατοστή was of, it was of something in gold, not current Attic silver.

Since Koehler until very recently, progress in understanding the documents was negligible.[6] That we now have any understanding of the texts at all, that they are a fit subject for submitting for your consideration today, is entirely due to the brilliant pertinacity and insight of Vladislav Andrejev of Leningrad. Working entirely from the printed texts, without even squeezes or photographs, he has made a number of observations which physical study of the stones does much to confirm and offered plausible hypotheses which make the texts eminently worthy of our closer study. Considering the difficulties under which he has worked, his achievement seems to me remarkable and must form the foundation for everything which is henceforth said on the subject. It will appear that there is much to be said which only emerges from study of the stones themselves, but I doubt if anyone would have thought the work worth doing, had it not been for the stimulus provided by his suggestions.

In his first article, in 1960,[7] he was primarily concerned with the contribution the texts could make to our knowledge of the price of land in Attica.

[4] *IG* ii. 2, p. 155. [5] *Attisches Recht* 740 n. 236.

[6] Kirchner's collection of the documents in *IG* ii² marked no advance, except that he had more fragments. He gave the fragments dates scattered through the fourth century. Two more fragments have been published since, by Schweigert, *Hesperia* 9 (1940), 330–2 and Peek, *AM* 67 (1942), 17–20. Peek added a fairly thorough textual revision of the previously known texts. Pritchett, *Hesperia* 25 (1956), 273, used the texts as a quarry for land-prices. A. W. Gomme left some unpublished notes on the texts which have from time to time turned out to be on the right lines.

[7] *VDI* (1960), no. 2, 47–57.

The first result of his operations was to add two fragments to our collection by pointing out that *IG* ii² 1580.*a* and *b* had been wrongly classified as ordinary *poletai*-documents (I note incidentally that Kirchner had dated these fragments *ante med. s.* IV). But this was a by-product of his major contention that the overwhelming majority of the sale-prices known to him were divisible by 50 or, more precisely, by 12.5. He was faced with || a number of apparent exceptions and made suggestions for disposing of them. Many of these suggestions I was able to confirm by study of our squeezes in Oxford, and you will find the full textual history of the inscriptions up to that point in *SEG* xxi 570–9.

From the dominance of 12.5 and 50 as multiples in the sale-prices Andrejev drew the conclusion that variations in the sale-prices could only be quantitative, that no qualitative distinction between different types of land could be being made. Such a quantitative distinction could only be related to area, and it followed that land was being sold at a single definite price irrespective of its quality. This price must have been arranged by the government. Land held by corporations could be cut up into properties of varying size, detaching units from an original block of land. Taking into consideration other evidence for the price of land in Attica, the price must have been fixed at 50 drachmae a plethron, with the quarter-plethron unit recognised and allowed for. State-intervention, Andrejev thought, was inescapable, but it cannot have been too unrealistic, so that one should assume that in fact agricultural land in general was remarkably uniform in price, and, given the supposed varying dates of the inscriptions, even from time to time. Even in 1960, he showed himself slightly sceptical about the dates of the inscriptions, and, particularly basing himself on the apparent arrangement of the documents by demes, none of which came twice, he allowed himself the tentative speculation that we were dealing with the consequences of a single measure, perhaps the consequence of the severity of Antipater.

In 1967,[8] he returned to these texts, this time within the framework of a study of Attic public land ownership. He reaffirmed the arrangement by demes and the failure of demes to recur, and suggested that there was also evidence of arrangement by tribes, and rightly thought that this indicated that the operation was much more likely to have been on a single occasion than the scattered publication of the texts suggests. He returned to his

[8] *VDI* (1967), no. 2, 48–76.

suggestion about Antipater, by which it now appears that he had thought that the sales by corporations had cloaked sales of formerly private property, and produced conclusive reasons for rejecting it, at least in this form. He now sees the operation as one designed to raise money, perhaps within the framework of Lycurgus' strengthening of public finances, and produces two contemporary literary texts to show that this was a possible Greek practice.

These texts will bear scrutiny, since I do not quite agree with his interpretation of them. The first is [Arist.] *Econ.* II.1346 B 13f.[9] The Byzantines, needing money, τὰ τεμένη τὰ δημόσια ἀπέδοντο, τὰ μὲν κάρπιμα || χρόνον τινά, τὰ δὲ ἄκαρπα ἀεννάως, τά τε θιασωτικὰ καὶ τὰ πατριωτικὰ ὡσαύτως, καὶ ὅσα ἐν χωρίοις ἰδιωτικοῖς ἦν · ὠνοῦντο γὰρ πολλοῦ ὧν ἦν καὶ τὸ ἄλλο κτῆμα. Andrejev says that ἀπέδοντο means 'sold', not 'leased'. My inclination is to say that it means both and that the distinction between χρόνον τινά and ἀεννάως makes this clear. There are some outright sales, but the productive land is only leased. Andrejev, in correspondence, is resistant to this view and finds it perfectly possible to envisage sales for a given period which would bring in more money immediately than a lease. Secondly, Andrejev leaves us with the impression that the state just confiscated the land of these religious organisations without compensation, but this is to ignore what follows, where the θιασῶται are granted or sold ἕτερα χωρία τὰ δημόσια near the gymnasion, the agora or the harbour, and certain monopolies and tax-collecting rights, on condition that they pay the state one-third of the proceeds. Clearly, in relation to these religious organisations, there was a complex deal. The state received hard cash for land, particularly for enclaves in private property which could be sold profitably, and gave the organisations in exchange extensive rights to future income. Only in the case of what is actually δημόσια does the state not have to provide anything in exchange.

The second text is *Rhet. ad Alex.* II.1425 B 19f., which says that a very common way of raising money is to see if there is any neglected *city* property which neither produces income nor is τοῖς θεοῖς ἐξαιρετόν, I mean, he says, τόπους τινὰς δημοσίους ἀμελουμένους, the sale or lease of which of private individuals may produce income for the city. I see no evidence here, though apparently Andrejev does, for any suggestion that the city might sell for its own advantage the property of communal organisations.

Andrejev adds various new observations of detail on our texts, but is

[9] Van Groningen's edition gives a full, not entirely satisfactory, commentary.

otherwise mainly concerned to use their evidence for his very full and suggestive treatment of the place of property owned by associations in the landpattern of Attica, which I fear I shall only touch on today. I now propose to leave him on one side for the moment and put before you the tentative results of a reconsideration of these fifteen fragments. The results are tentative because I have not actually looked at the stones since 1962 and, although I have had the squeezes before me from time to time since then, there are some points about the arrangement of the fragments which I would ideally like to look at again. Large patches of the fragments are distinctly hard to read and the script even on the same fragment is very far from uniform. Much of the past failure to recognise that fragments belong together is probably due to the fact that lettering at the beginning of a line may look a great deal tidier than the end of that same line. Close inspection of the squeezes and stones nevertheless permits much greater precision about individual transactions and the nature of || the operation as a whole. Appendix A attempts to summarise the evidence now available, and I now pick out some of the salient points.

The most important point is that I see no essential obstacle to grouping the fragments in only three stelai and that I see no reason to doubt that these stelai are very close in date. Andrejev's suspicion that we are dealing, not with routine operations spread over most of the fourth century, but with something which is happening virtually on only one occasion, seems to me amply confirmed. What the date is I cannot affirm with certainty. Epigraphically, there seems to be some similarity to the approach to the inscribing of documents with that found in some of the texts of the φιάλαι ἐξελευθερικαί, probably in the decade 330–320,[10] and that found in some of the accounts of the Treasurers of Athena early in the next decade. The periods of Lycurgus, Antipater and Demetrios of Phaleron all seem to me possible. As far as I can see, the prosopographical evidence, which is not remarkably strong, gives a similar picture and allows no greater precision.

Andrejev's further suspicion that a tribal order is detectable also receives considerable substantiation. However, this must not be pressed too far. There is no indication of any tribal heading, even for example in *IG* ii² 1597.A, Col. II where we would think we see a distinct change from the fifth to the sixth tribe in the official order, and I think there is more evidence than Andrejev allows for the complications which would be caused by the same

[10] *Hesperia* 28 (1959), 236f.; 37 (1968), 372.

corporation holding property in more than one deme or even tribe. Nevertheless, the working principle of looking for tribal order produces fairly satisfactory results. I would not like to exclude the possibility that I have been led into circular argument from time to time in my fragment placing, but, to take one obvious example, it is comforting that 1594 shows property in Poros (Tribe v) in the left-hand column of one face and property in Phaleron (Tribe ix) on the right-hand column of the reverse, and that 1595 shows purchasers apparently from another deme of Tribe v and property in Tribe ix in the corresponding columns of the same stele. As we shall see, however, there appears to be more than one tribal sequence.

The only fragment covering the full width of a stele (1597) has two columns on a face. I have had occasional doubts about applying this fact to the other two stelai and there may be grounds for saying that such a supposition would make the second stele in particular rather tall. But this stele has the same thickness as 1597, which makes a prima facie case for giving it roughly the same width. I propose to operate a two-column hypothesis throughout until counter-evidence appears.

To a first stele, of Pentelic marble with a marked tendency to split, || substantially *stoichedon* on both sides, I attribute five fragments (1580 (2), 1601 (2), 1603). In Column i of the obverse, we appear to be in Tribe i. Gomme acutely guessed the presence of property at Lamptra and above it we see a purchaser from Euonymon, of the same tribe and indeed of the same trittys. In Column ii we have sellers restorable as Κολω[νεῖς] and Φηγ[αιεῖς]. This is consistent with the possible presence of Tribe ii, though there are difficulties.[11] In another fragment of Column ii a single selling unit (no. 6) disposes of property both at Prasiae and at Paiania. These are both in the inland trittys of Tribe iii; the selling unit was not, I think, a deme, since I cannot believe in a deme holding property in another deme. Fragments (nos. 7 and 8) from uncertain positions on this face put us in Tribe ii and, much less certainly, in Tribe iii. The only fragment of the reverse (nos. 9–11) apparently indicates Tribe v in both its columns, though conceivably the inference is unsound for the first column.[12]

[11] Κολωνεῖς is so far only attested for the demes Kolone in Tribes iv and x; we do not know how the members of the deme Kolonos (*BSA* 50 (1955), 12–17) would have described themselves in the plural [in bouleutic lists they regularly use ἐκ Κολωνοῦ]. Φηγ[could be Φηγ[ούσιοι] (1).

[12] I am satisfied that *IG* ii² 1601.16–17 should be read [Σ]φ[η]ττ[ίων δήμαρχος] | Ἀ[ντ]ίπα[τρος ἀπέδοτο χωρίον] (cf. *PA* 11178) and at least four of the properties are

This first stele, then, appears to cover property substantially in Tribes I–V. Four fragments (1594, 1595, 1598, 1602) go to make a second stele. This one begins in Tribe V, as far as we have it, though it is uncomfortable, as Andrejev saw, to find a property at Be[sa] (X) intruding in a list of property at Poros (no. 12). Further down Column I (nos. 14, 15) we appear to be already in Tribe VII. Column II begins with an utterly intractable entry (no. 16) where the same sellers dispose of property in at least four different locations, none of them very easy to read. The weight of the evidence may suggest that we are already in Tribe X, but I retain a suspicion that the presence of a multiple purchaser from Melite (VII) may point another way. The next entry (no. 17) puts us firmly in Tribe VII with the start of an entry from Halai Aixonides, the first case, incidentally, where we can be sure that a deme and not some other organisation is the seller. The reverse of this stele is firmly based in Tribe IX with sales mostly by territorial organisations. In Column I we are in the neighbourhood of Phaleron, with sales by κῶμαι which are apparently sub-divisions of the Old Tetrakomoi.[13] In Column II there are sales by similar κῶμαι in Aphidna, followed by the deme of Oinoe. Some of these were later || promoted to a full deme-status which they did not enjoy in the classical Athenian structure,[14] and it is interesting to observe that Column II ends with two more designations, Μελαινέ[ων] and Κλωπιδ̣[ῶν], which were similarly promoted,[15] without our being able to guess where their location was. We can only say that this stele covers tribes from V to at least IX.

These two stelai, to all appearance, have a fairly continuous structure. Territorial organisations, of a sort, appear to be dominant. Twenty-seven of the thirty-one sellers now represented are disposing of more than one property. Only four have only one, and for at least one of these (no. 21), there is a clear organisational reason for its being listed here. There are no grand

purchased by Sphettians. The inference for the left-hand column depends on at least two and perhaps five of the properties being purchased by demesmen of Cholargos (V), but I do not recognise the locative ending in]ίδι which appears certainly in 1601.10 and probably 1601.8.

[13] The situation is not quite clear; see Roussel, *RA* 18 (1941), 226–31; Lewis, *Historia* 12 (1963), 33 with n. 104 = this volume, 90 with n. 105. The recognition of [Θυμαι]ταδῶν in *IG* ii² 1598.13 and its consequent restoration in line 10 strengthens my view.

[14] Hyporeia and Petalidai are attested for Ptolemais.

[15] They are both attested for Ptolemais. If one could believe Steph. Byz. *s.v.* that Melainai had earlier belonged to Antiochis (X), it would be very satisfactory. But there is no other evidence for it. [On the post-Cleisthenic demes see now Traill, *The Political Organization of Attica* 87–95, 113–22.]

totals visible. The third stele, which seems to take in the remaining six fragments, presents a rather different picture. Except in the case of one very dubious reading I made from the stone and cannot confirm from the squeeze (no. 48), there are no territorial organisations selling. The ascertainable sellers are all cult-organisations of one kind or another, and a high proportion of the responsible officials are called ἐπιμεληταί. Of the twenty-seven surviving sellers, at least fifteen have only one property to dispose of. Besides the separate calculations of totals and ἑκατοσταί, two grand totals are preserved. This stele seems therefore to present slight differences from the other two. Nevertheless, the tribal order is still relatively visible. Column I of the obverse contains properties in Tribe II, Column II properties in V, VI and X. Column II ends with a total, 20 talents, 3,644 drachmae, 5½ obols which it is reasonable to attribute to the whole of this face. In Column I of the reverse we have a very illegible fragment (nos. 47, 48) which appears to mix properties from V and VI, another (nos. 49, 50) with two properties from VII, and we end in Tribe X followed by another total of 13 talents, 3,300 drachmae. Below this, still in Column I, there is a single entry (no. 53) from Salamis, outside the Athenian tribal structure. Of Column II little is certain, save that one fragment (nos. 54, 55, 56) shows two properties in Tribe VII. If there are tribal sequences in this stele, there are at least three, and this remains true even if one reverses the order of the faces.

There is one more epigraphic contribution which should not be overlooked. The pattern of finding of the fragments is unequivocally that of texts which were originally set up on top of the Acropolis, and is sharply distinguishable, for example, from that of the documents of the *poletai*. I suggest that, just as the tribute-lists were on the Acropolis because of Athena's one sixtieth, just as the lists of the φιάλαι ἐξελευθερικαί were there because the φιάλαι came to Athena, so our texts were there because || the ἑκατοσταί came to Athena, and I submit that this must mark a final clear distinction between our texts and the procedure recorded by Theophrastus. This inference is the only one which can be safely drawn from the find-spots, which have, I think, nothing to tell us about the officials responsible for the transactions.

We can now return to thinking about the meaning of the texts. First, you will want to know the current position about Andrejev's base-unit of 12.5 drachmae. I think he can rightly claim that the results are very satisfactory. I have left the total in no. 16 with Peek's readings as 2 talents, 630 drachmae, but one could not exclude 2 talents, 637½ drachmae. In no. 9 my repeated

best endeavours with the left-hand column of 1601 still leave me feeling that the most likely readings give three intransigent figures of 97 dr., 115 dr. and a figure ending with 22½, but I freely admit that the stone is much damaged and that I may be wrong. No. 20 shows 440 drachmae, a figure which is immediately taken over into a total of 2 talents, 40 drachmae and is apparently intractable. The ἑκατοστή is given as 120 drachmae, 2¼ obols, which is strictly the ἑκατοστή on 2 talents, 37½ drachmae, which would imply a sale price of 437½ drachmae, which is divisible by 12.5, but I cannot see how it could happen that the ἑκατοστή could be right and the individual sale-price and the total both wrong. There is also to be considered the total on Stele III, Face A, 20 talents, 3,644 drachmae, 5½ obols. Andrejev most ingeniously explains this by supposing that a ἑκατοστή has been added in by mistake. No one with any experience of Athenian public arithmetic will find this implausible.

As far as I can see, then, Andrejev's basic contention that virtually all prices are divisible by 12.5 drachmae almost certainly stands. What of his deduction that all property on these texts was sold at a fixed price per plethron, on state-intervention? I may be a victim of a modernist fallacy, but, though I find myself nowadays more capable of believing improbable things about ancient Greece than I did when I was younger, I must say that I still find the picture of all land being held to be of the same value as revolting today as I did when I first read Andrejev's first article. English prices for agricultural land today, I am told, vary by a factor of 20. I cannot believe that the view that all Attic land was of the same value can have been held except by a lunatic or, just conceivably, a philosopher. What machinery can we envisage for deciding who was going to get the land at this fixed price? What advantage could the responsible authorities see in fixing a price rather than having a sale by auction? One would suppose from one's general knowledge of fourth-century Athens that the responsible officials would wish, even if they were not legally forced, to have a sale by auction, purely to avoid accusations of || bribery.[16] Consider a text like *IG* ii² 1183, on the possessions of the deme of Myrrhinous, with its careful assumptions that absolutely no one is to be trusted, and compare our texts where twice (nos. 17, 39) the official responsible for selling a property is actually the purchaser, in one case of a four-talent property. If this was done at a fixed price, the officials concerned

[16] I owe this point to G.E.M. de Ste Croix.

must have been very confident of their probity or living under a regime where they had no need to care what was thought of them.

I once put to Andrejev the alternative possibility that these were sales by auction and that the periodicity of the figures was due to the auction being conducted in fixed steps. I quoted at him a contemporary English auction-catalogue 'No person to advance less than 1 shilling [= £0.05]; above five pounds, five shillings; and so on in proportion', and said that in this case all prices below £5 will be divisible by one shilling, all prices between £5 and £10 by five shillings and so on. To this he rightly replied that there was no trace of such a periodicity in any Athenian sales which were certainly conducted by auction and, that if one could sell a plot and a house at Prasiae for at least 1 talent, 2,012½ drachmae (no. 6), there was no trace either of the intervals getting larger as they do in contemporary auctions. (The occasional appearance of houses in the documents is of course a complication of Andrejev's scheme, as he readily recognises.)

I do not know how far I shall find support for my distaste for fixed prices. I must admit that neither of those who have read this paper before the meeting have much sympathy for it. Finley writes: 'How did Greek land interest rates manage to remain stable for centuries at 1 drachma per mina per month, without usury laws? Ancient prices did not always behave like modern ones, and especially not when the state was involved. I do not understand the procedure any more than you do, but it cannot be simply lunatic to think that, under these unusual circumstances, whatever they were, there was a fixed price per plethron regardless of quality of the soil.' Andrejev makes some new and helpful points. He feels I am drawing a false analogy from privately owned land. These properties we are dealing with are ones in which citizens did not have any direct personal interest. In the ecclesia they could accept even absurd resolutions and look at the matter from the point of view of prospective buyers. Secondly, and I am not myself sure that this does not to some extent counteract his first point, he feels, and surely rightly, that at any rate the territorial organisations we are dealing with here have much less corporate reality than in the modern world. Communal land was the possession of a collective rather than of a municipality. The members of the collective might distribute the lands among themselves even without payment, provided that someone convinced them that this would be to their advantage. || Thirdly, his feeling about the whole transaction is that nothing like all the land is being sold. If they are selling off abandoned, remote and

inconvenient land, a fixed price might seem most reasonable and realistic. Lastly, he is prepared to abandon his view of a single fixed price in favour of more than one fixed price, perhaps 12.5 drachmae a plethron for those properties described as *eschatiai*, 50 drachmae for those described as *choria*. I have reviewed the evidence in the light of this and there may well be something in it. The only case where 12.5 is a multiple with a *chorion* is no. 6, lot 3, which we have already looked at, and where a house complicates matters. Some *choria*, however, still show a multiple of 25. No. 6, lot 2 has a house; no. 35, lot 4 and no. 56, lot 1, however, do not.

I leave the problem open for discussion and only point out, besides what seems to me the basic implausibility of the idea that all land is of equal value, the considerable administrative difficulties involved in verifying the sizes of the plots and determining who was going to get the plot at the fixed price. I can however throw some light on the last problem. During and after the last war, England had price-control on furniture and this applied to auctions as well. It not infrequently happened that several people, knowing the scale of fixed prices, bid the maximum simultaneously. The problem was resolved in a good Athenian way; the bidders drew lots. However, there was some suspicion that buyers on good terms with the auctioneer tended to be frequently successful in the lot. I am not prepared to assert that the suspicion was unfounded.

Of course, even if we dismiss the idea that the price was fixed by the state, state-intervention in these transactions is still certain. It did not occur to at least 58 corporate sellers spontaneously and simultaneously to sell off their properties, and the transactions were put on public record on the Acropolis. As far as I can see, there are three main reasons why there might be state-intervention of this kind. One is purely financial, to bring the state money. Another would be social, to increase the amount of land in private cultivation and satisfy land-hunger among those who did not possess it. The third is hardly different from the second, except in its beneficiaries. It might well be in the interests of an oligarchic regime to use state power to benefit rich citizens who had long had their eyes on patches of land owned by organisations which would round out their own estates. The example of Byzantion,[17] of course, combined the first and third reason. Money would be more easily raised on properties which did round out other estates.

If we are going to accept fixed prices for land, this would seem to me prac-

[17] See pp. 189–90 = this volume, 266.

tically to rule out any situation where the first, financial, motive is primary, for then the government would wish to maximise the profit by || auction. Either of the other two motives would still be possible. We ought to get somewhere towards greater understanding if we look more closely at the individual transactions, but the results are hardly conclusive. If we take only those individual prices which are fully preserved, 8 fall in the range 1–4 T., 5 in the range 3,000 dr.–1 T., 6 in the range 1,000–3,000 dr., 7 in the range 500–1,000 dr., 11 in the range 100–500 dr., 12 in the range 25–100 dr. The average of these figures is 3,214 dr., but the median is only 525 dr., and one gets much the same figure for the median if one adds in the fragmentary figures which can properly be used for it. In other words, there were some quite large properties, but there were a great many very small ones too. A large proportion of these small properties come in two main groups, nos. 9 and 25. Of no. 9 we can say little, but the small properties in no. 25 are *eschatiai* in the κώμη Hyporeia of Aphidna. I see no evidence which would enable us to say whether or not these were naturally small lots or artificially created for the purpose of the sale.

A little more comes from observation of the purchasers. I see no way of giving you meaningful percentages, but there are six cases so far known of purchasers who bought more than one lot, and at least twenty-four cases of purchasers who look fairly local and may have had other land in the neighbourhood. This last of course fits the Byzantion pattern, but would be fairly commonplace in land sales of any type.[18]

For what it is worth, my impression is that we do not have here a picture of either of the social reasons for selling off land held by organisations and that it is much more likely that the reasons are purely financial. What is the evidence that the state rather than the organisations profited by the sale? The only argument one might invoke is the appearance of grand totals covering a considerable number of sellers on the third stele, and even that might be explained as purely a check on the calculation of the ἑκατοστή. I think it is likely that the state profited, but I do not see how one can prove it, nor is there any trace of what the organisations would receive in return. Simple confiscation of property seems very improbable to me.

What of the organisations themselves? As Andrejev has clearly shown in

[18] I have in mind e.g. *IG* ii² 1579.1–7, where I have always suspected that the purchaser is identical with the neighbour quoted to identify the boundary of the property.

his second article, their main interest would certainly not be in the capital value of their property but in its ability to produce income to maintain their cult activities. In 321/0, for example (*IG* ii² 2498), we find the deme of Piraeus reviewing the conditions of its leases. Any offer on the Byzantine model which might offer the deme increased income might well seem attractive. There is, however, a counter-example from a little later, in 300/299 (*IG* ii² 1241), where the phratry of the Dualeis includes || in a ten-year lease of a plot at 600 drachmae a year an option for the lessee to purchase the plot outright for 5,000 drachmae. A great deal has been written about the relationship of these two figures, but no one to my knowledge has asked the question, what was the phratry going to do with the 5,000 drachmae, if it got it? What alternative method of investment did it have in mind? I ask the question now, and offer no answer.

I have a little to add to Andrejev's treatment of the picture of organisationally held property presented by these texts. Of the fifty-eight selling organisations, only ten so far can be held with reasonable probability to be territorial. Twenty-seven certainly were not. The proportion of organisations and cults not known at all from other sources is remarkably high and one more index to the very great complexity of classical Athenian social life. The amount of property held by these organisations varies, but is in some cases very high. The eighteen totals fully preserved show a range from 50 drachmae to 10 talents, 5,950 drachmae, an average of 5,835 drachmae and a median of 1,000 drachmae, but this is certainly misleading, since eight out of the thirteen incomplete totals exceed one talent. The two grand totals on the third stele, even though it is mainly concerned with smallish cult properties, add up to over 34 talents. The sequence of the first two stelai shows roughly the same amount even in its present fragmentary state and must have run considerably higher. If I had to guess at the amount of property sold on this occasion, I would guess something in the range 200–300 talents. We cannot of course be certain that this represented all organisationally held land, let alone the full value of all property held by organisations.

The τίμημα of Attica for *eisphora*-purposes in the 350s, which certainly included the property of organisations, was 6,000 talents.[19] Some of these properties will have fallen below the exemption-limits, but will not be quantitatively very significant. Let us say that the land passing through these transactions

[19] Dem. XIV.19.

formed something like 2–5 per cent of the τίμημα of Attica. The only possibility of getting any kind of check on these figures lies in what Demosthenes has to say in XIV.16 about getting a list of 1,200 efficient trierarchs. He recommends extending the trierarchic list to 2,000. Excluding from the 2,000, he says, the *epikleroi*, the orphans, the unfit and, what interests us, τὰ κοινωνικά, we will get 1,200 σώματα. Demosthenes will then be saying, and it can hardly be more than an educated guess, that 40 per cent of the largest properties in Attica will turn out to be at any one time in the hands of one of these four categories. It is clear that there might be quite a large number of properties falling under the other three categories. Something like 5 per cent of the whole number for τὰ κοινωνικά will clearly not be unreasonable, but this is a percentage of the number of || properties falling above the trierarchic qualification. It is not strictly comparable with the guess that Andrejev,[20] basing himself on some very slender hypotheses, makes of something under 10 per cent for the amount of Attic land, by value and area, held by organisations. We are agreed, I think, that organisationally held land formed a significant, but in no way dominant, part of the pattern of landholding in Attica. I see no reason to believe that our transactions resulted in any very major change in that pattern, to the benefit of either rich or poor.

Various topics remain for discussion. I have not dealt with the location of organisationally held property, which Andrejev has discussed very fully, nor with what seems to me the remarkably stereotyped descriptions of property in these texts. Beyond a few houses and possibly a κῆπος, everything in these texts is either a χωρίον or an ἐσχατιά. If we contrast the descriptions of property in the confiscated property of the Hermokopidai,[21] the poverty of the language employed here seems remarkable, but Finley reminds me that the language of *horoi* is hardly more varied. In Appendix C I discuss the meaning of ἐσχατιά in Attica, and the distinction drawn here must have real significance, which adds some attraction to Andrejev's latest thoughts about a differential fixed price. But whatever its implications, I call your attention to the very large price, 1 talent, 3,100 drachmae, fetched by an ἐσχατιά at Lamptra (no. 2, lot 1). On Andrejev's latest suggestion, this brings the area out at 728 plethra = 64 hectares = 157 acres, which seems to me rather large.[22]

[20] *VDI* (1967), no. 2, 72. [21] *Hesperia* 25 (1956), 261–9.

[22] For comparative material, see de Ste Croix, in *Ancient Society and Institutions . . . V. Ehrenberg* 109–14.

APPENDIX A

Stele I (*IG* ii² 1580 + 1601 + 1603)

FACE A

Publication	*Index no. of seller*	*Seller and number of officials*	*Kind of property*	*Location*	*Deme of purchaser*	*Price*
COL. I						
1603	(1)	?	?	?	Euonymon	4,050
			unaccountable]ου			
		Total (restored from *hekatoste*): 10 T. 5,950 dr.			Tribe I?	
	(2)	[Λάμπτρα	ἐσχ.	Lamptra	two (one]TTI)	1 T. 3,100
		κ]αθύπερθε (1)	?ἐσχ.	?Lamptra	?as above	?
		Total: at least two properties:	1 T. 3,100+		Tribe I	
COL. II						
1603	(3)	?	?	?	?	?
			ἐσχ.	?	?	?
		Total: at least two properties:	?			
	(4)	Κολω[νεῖς](1)	?	?	?	?
		Total: one property only:	?		Tribe II?	
	(5)	Φηγ[ ](3+)	?	?	?	?
		Total: at least one property:	? (Not deme, since plural sellers)		Tribe II?	

Publication	*Index no. of seller*	*Seller and number of officials*	*Kind of property*	*Location*	*Deme of purchaser*	*Price*
1580a	(6)	?	χωρ. καὶ οἰκ.	?	?	500
			χωρ. καὶ οἰκ.	Prasiai	two (one Ἀθμ)	]XXHHHΡΔΔΓ
			χωρ. καὶ οἰκ.	Prasiai	one	]TXXΔHHIII
			χωρ. καὶ οἰκ.	?	Myrrhinous	5,000
			χωρ. καὶ οἰκ.	Paiania		]ΡHHH
		There follows]ΔΔΓHH at end of a line, probably *hekatoste* on the total				
		Total: at least five properties, all with houses, in III Mesogeia: minimum 2 T. 4,687½				||
UNCERTAIN COLUMN						
1580b	(7)	?	?	?	three (one Τειθ)	?
			χωρ.	Teithras	one	?
		Total: at least two properties: 1 T. 3, 050			Tribe II	
1601a	(8)	?	At least four properties, one purchaser perhaps Μυρρ. (III), no prices.			
FACE B COL. I						
1601	(9)	?	?	?	?	70+
			?	?	?	97+
			?	?	?	115
			?	?	?	70

			?	?	?	62½
			?	?	Cholargos (v)	62½
			?	?	Cholargos	25
			?	]ιδι	?	50
			?	]ιδι	?	]ΔΔΗΗΙΙΙ
			?	?	?	]ΔΔΓ
		After two blank lines mysterious]ΓΗΗΗ				
		Total (ignoring last entry); at least ten properties: minimum 599½				

COL. II

1601	(10)	?	probably more than one property			
	(11)	Deme of Sphettos (?)	[χωρ] καὶ οἰκ.	[Sphettos]	Sphettos	1, 000+
			χωρ	[Sphettos]	the same	?
			χωρ	[Sphettos]	the same	?
			χωρ	[Sphettos]	?	?
			χωρ	[Sphettos]	Aphidna	?
			χωρ	[Sphettos]	[Sphettos]	?
			χωρ	[Sphettos]	?	?
		Total: at least seven properties: minimum 1,000			Tribe v?	||

Stele II (*IG* ii² 1594 + 1595 + 1598 + 1602)

FACE A

Publication	*Index no. of seller*	*Seller and number of officials*	*Kind of property*	*Location*	*Deme of purchaser*	*Price*
COL. I						
1594	(12)	?	(χωρ)	?	?	?
			χωρ	Poros	?	?
			χωρ	Poros	?	?
			ἐσχ	Poros	same	?
			χωρ	Poros	Ψ[	?
			ἐσχ	Besa	Halai	?
			ἐσχ	Poros	Halai	125
			ἐσχ	Poros		
			ἐσχ	?	?	?
		Total: at least four χωρία and five *eschatiai*. 6 properties in Poros (v), 1 in Besa (x), 2 uncertain. Not a deme since more than one location. Poros assigned to Asty by Milchhoefer, but this is not certain.				
1595	(13)	?	(χωρ)	?	?	?
			χωρ	?	?	?
			χωρ	?	Thorikos (?)	?
			χωρ	?	?	?
			χωρ	?	the same	?

			χωρ	?	Thorikos (?)	?
			χωρ	?	the same	?
			χωρ	?	?	?
		Total: at least least eight *choria*. Thorikos (?) (v)				||
1602. 21–31	(14)	?	?	?	Halai (VII?)	1,000
			?	?	Halai (?)	600
		Total: at least two properties. Total ends in]III minimum 1,600				
	(15)	?	?	Phlya	Paiania	2 T. 3,000
			(χωρ) και οικ	?	two?	
					(1 Skambonidai)	]XX[-]HI[-
		Total: apparently two properties only, minimum 3 T.			Tribe VII?	
COL. II						
1598B	(16)	?	(χωρ)	?	Melite	?
			χωρ	?	the same	?
			χωρ	Kroia (?)	?	?
			χωρ	Echelidai (?)	?	?
			χωρ	?	?	?
			χωρ	Ἀνα (?)	?	?
			χωρ	Am (phitrope)?	the same	?
		Total: at least 7 χωρία: 2 T. 630 dr. Not a deme since more than one location. Locations doubtfully read, all except Echelidai tribe X.				

Publication	*Index no. of seller*	*Seller and number of officials*	*Kind of property*	*Location*	*Deme of purchaser*	*Price*
	(17)	Deme of Halai (Aixonides)	χωρ	Halai	the demarch	4 T.
			χωρ	Halai		
			χωρ	Halai	?	?
		Total: at least 3 χωρία, minimum 4 T.			Tribe VII	
1602	(18)	?	at least 5 χωρία(?), one ἐν Σ[, buyers all different. No figures.			‖
FACE B						
COL. I						
1598A	(19)	?	?	?	Paionidai	100
			(χωρ)	Phaleron	Xypete	150+
			χωρ	Phaleron	Piraeus	300
		Total: 3 + properties: 3,575 dr.			Tribe IX	
	(20)	Archontes of the kome Thymaitadai (2, from Phaleron)	χωρίον καὶ οἰκόπεδον	Thymaitadai	Thymaitadai	1 T. 5,600
			χωρ.	Phaleron	Piraeus	440
		Total: 2 properties: 2 T. 40 dr.			Tribe IX (Phaleron)	
	(21)	Archon of the kome? (1, from Phaleron)	χωρ	Phaleron	Sypalettos	1,000
		Total: 1 property only: 1,000 dr.			Tribe IX	

	(22)	]ερος Κωλιέων	?	Phaleron (?)	Phaleron	?
		Total: at least 1 property.			Tribe IX	
1602A	(23)	?	?	?	?	]XX
			?	?	Oinoe	3,500
		followed by two or three properties		[ἐν'Ελευσῖ]νι		
		Total: 4–5 + properties: minimum 1 T.			Tribe VIII? IX?	
COL. II						
	(24)	?	?	?	]αι	125
		Total (restored from *hekatoste*) 1,000. Therefore more than one property.				||
	(25)	[Ὑπωρείων κωμαρχ]ός (1, from Aphidna)	ἐσχ	Aphidna in Hyporeia	Aphidna	50
			ἐσχ	Hyporeia	Aphidna	50
			ἐσχ	Aphidna in Hyporeia	Aphidna	50
			ἐσχ	Aphidna in Hyporeia	the same	500
			ἐσχ	Aphidna in Hyporeia	Aphidna	50

Publication	*Index no. of seller*	*Seller and number of officials*	*Kind of property*	*Location*	*Deme of purchaser*	*Price*
			ἐσχ	Aphidna in Hyporeia	ἐκ Κολωνοῦ	50
			ἐσχ	Aphidna ὑπὸ τῶι Φανερῶι		
		Total: 7 *eschatiai*: 750			Tribe IX	
	(26)	[Πεταλι]δῶν κωμαρχός (1, from Aphidna)	ἐσχ	Aphidna in Petalidai	Aphidna	500
			χωρ	Aphidna in Petalidai		
		Total: 2 properties: 500			Tribe IX	
	(27)	[Οἰνοαίων] δημαρχός	χωρ	Oinoe	Oinoe	550
			χωρ	Oinoe	Oinoe (?)	300
		Total: at least 2 properties: minimum 850.			Tribe IX	
1595B	(28)	?	?	?	]ο	3 T. 2,000
			?	Rhamnous	Rhamnous	3,000
			?	Rhamnous	Rhamnous	3 T. 1,000
			?	?	?	?
			?	Rhamnous	]ν	200
		Total: at least 5 properties: minimum 7 T. 200 dr.			Tribe IX	

1602A	(29)	]ΟΙΑ[(multiple officials)	?	?	two	?
		Total: one property only?				
	(30)	Μελαινεῖς (1 official)	?	?	?	?
		Total: one property only				
	(31)	Κλωπίδαι (1 official)	?	?	?	?
		Total: at least one property				

Stele III (*IG* ii² 1596 + 1597 + 1599 + 1600 + Ag. I 3771 + *AM* 67 (1942), 17–20

FACE A

COL. I

1599	(32)	?	end of entry with [κ]εφ[, so more than one property			
	(33)	[ὀ]ργε[ώνων] (3 *epimeletai*, 1 Kerameis, 1 Thymaitadai)	χωρ	?	?	?
		Total: one property only				
	(34)	[Ἥρ]ω Ἀλκιμάχου	?	?	?	?
		Total: number of properties uncertain				‖

Publication	*Index no. of seller*	*Seller and number of officials*	*Kind of property*	*Location*	*Deme of purchaser*	*Price*
1597	(35)	?	?	?	Phrearrhios	?
			ἐσχ	Kydantidai	Kydantidai	162½
			χωρ	Kydantidai	Kydantidai	1,000
			χωρ	Kydantidai	Kydantidai	2,275
		Total: at least 4 properties:	4,837½ dr.		Tribe II	
AM 67 (1942)	(36)	? (at least 3 officials)	χωρ	?	Euonymon (1)	1 T. 2,000 dr.
		Total: 1 property only: 1 T. 2,000 dr.				
	(37)	[Ἡρακ]λέους Πλουσίου (1 official)	χωρ	?	?	?
			χωρ	?	Alopeke	?
		Total: at least 2 properties			Tribe X?	
COL. II						
1597	(38)	? (not a deme)	ἐσχ	Kephale	Kerameis	62½
		Total: 1 property only: 62½ dr.			Tribe V	
	(39)	Οἰκάται (*epimeletes* from Kothokidai)	χωρ	Kothokidai	The *epimeletes*	100
		Total: 1 property only: 100 dr.			Tribe VI	

	(40)	Ἀφειδαντίδαι (*epimeletes* from Epikephisia)	χωρ	Kothokidai	Kothokidai	250
		Total: 1 property only: 250 dr.			Tribe VI	
Ag. I 3771	(41)	? end of entry only, very dubious				
	(42)	Hephaistos in Lousia (official from Melite)	χωρ	Lousia	Acharnai	2,600
		Total: 1 property only: 2,600			Tribe VI	
	(43)	]AKINIΔ[	?	?	?	?
		Total: number of properties uncertain				
1596	(44)	?	κῆπος	Pallene	Pallene	250
		Total: 1 property only: 250			Tribe X	
	(45)	]πιλυσίου (*epimeletes* from Pallene)	χωρ	Pallene	Pallene	50
		Total: 1 property only: 50				
	(46)	? (*epimeletes* from Anaphlystos)	χωρ	Anaphlystos	Sounion	800
		Total: 1 property only: 800			Tribe X	
		GRAND TOTAL: 20 T. 3,644 dr. 5½ ob.				

End of column

FACE B

Publication	*Index no. of seller*	*Seller and number of officials*	*Kind of property*	*Location*	*Deme of purchaser*	*Price*
COL. I						
1597	(47)	?	?	?	?	]ΡΔΗΗΙΙΙ
reverse			χωρ	Acharnai (VI)	Azenia	4,000
			?	Cholargos (V)	Cholargos	?
		Total: at least three properties: *hekatoste* suggests sum ending with 3,750 dr.				
	(48)	This entry is very hard to read: I once thought I read the seller as the deme of Acharnai, but I now think this very unlikely on grounds of spacing. The first property included an οἰκία. An isolated figure]ΧΧΔΔ[is visible further down.				
Ag. I 3771	(49)	?	ἐσχ	Phlya	Lamptra	1875
		(more than 1 official, the last from Athmonon)				
		Total: 1 property only: 1,875			Tribe VII	
	(50)	Διπολιασταί				
		(2 officials, 1 Phlya)	χωρ	Phlya	?	?
		Total: at least 1 property:	?		Tribe VII	
1596	(51)	?	?	Alopeke	two (1 Teithras)	?
		Total: 1 property only:	?		Tribe X	

	(52)	Herakles (two *hieromnemones*, one Alopeke)	χωρ	Alopeke	Atene	1 T+
		Total: 1 property only, between 1 and 5 T.			Tribe x	
		GRAND TOTAL: 13 T. 3,300 dr.				
	(53)	Eikadeis on Salamis (*boularchos* from Π[)	χωρ	Salamis at Chytreai	Oion	2,000+
		Total: 1 property only, between 2,000 and 5,000 dr. Outside tribal structure.				
End of column						
COL. II						
1597 reverse	(54)	At least three χωρία one purchaser from Euonymon (1)				
AM 67 (1942)	(55)	?(multiple officials, 1 Xypete)	χωρ	Xypete	Xypete	?
		Total: 1 property only: ?			Tribe VII	
	(56)	? (2 officials, both Athmonon)	χωρ	Athmonon	Athmonon	625
			χωρ	Athmonon	Athmonon	?
			χωρ	Athmonon	?	?
		Total: at least three χωρία minimum 625.			Tribe VII	
UNCERTAIN COLUMN						
1600	(57)	? Total: probably more than one property: 1,350 dr.				
	(58)	A phratry	ἐσχ	?	?	?
		Total: at least one property: ?				‖

APPENDIX B

I should have mentioned the Hadrianic inscription *IG* ii² 2776, a list of properties with the standing formula (1) names of individuals in the nominative, (2) description of plot in genitive, (3) location of plot, (4) sum of money. As Mommsen (*Hermes* 5 (1871), 129–37) observed, the sums of money show periodicity with widespread use of the multiples 12.5 and 6.25. He concluded that the individuals were accepting a liability to pay for unknown purposes 8 per cent of the sums recorded. There are several other possibilities, fully discussed by John Day, *Economic History of Athens under Roman Domination* 221–30, who leans to the view that the sums recorded are themselves percentages of capital values and are themselves the amounts which the individuals are obliged to pay. I have considered various ways of giving our texts an analogous solution which might produce capital sums on which the state could draw and income which the organisations would require, but see little prospect of reconciling such a solution with our texts' straightforward sale-formulae, however fictitiously (see now Miller, *Hesperia* 41 (1972), 67–93).

APPENDIX C

For the meaning of ἐσχατιά, historians generally refer to Wilhelm, *Neue Beiträge* iii. 13–14. He throws considerable emphasis on the aspect of the word which involves being 'away from the town' and I can see that this has attractions from the point of view of colonial and new settlement-patterns. It does not, however, appear to me to give a balanced picture of actual Attic usage, and I am comforted to see that Gow on Theocritus, xiii. 25, investigating general literary usage, also has rather a different view.

Of course, ἐσχατιά can mean 'land on or towards the frontier'. This is clear enough for Plato, *Laws*, VIII.842E and Aristotle, *Politics*, VII.1330 A 9f. Lexicographers occasionally refer to the frontiers in their definitions: cf. Bachmann, *Anecd.* i. 238. 4; *Suda*, E 3253, and Bekker, *Anecd.* i. 256. 30: ἐσχατιαί · τὰ χωρία λέγουσιν ἐσχατιὰς τὰ πρὸς τοῖς ὅροις τῆς χώρας ἢ τὰ πρὸς τῇ θαλάσσῃ. One text asserts frontiers in relation to demes, the second definition in *Schol. ad* Aeschin., p. 271 Schultz = p. 38 no. 211b Dilts: τὰ ἐπὶ τοῖς τέρμασι δὲ τῶν δήμων ἔσχατα κείμενα χωρία ἐσχατιαὶ ἐκαλοῦντο.

Some ἐσχατιαί in real life may conform to this definition. It is probably correct for Xen. *Hell.* II.4.4, where the Thirty, fearing plundering by the men of Phyle, send out the Spartan garrison and two tribes || of cavalry εἰς τὰς ἐσχατιάς about 15 stades from Phyle and they camp ἐν χωρίῳ λασίῳ. One could not be clear whether the ἐσχατιαί are the extremities of Attica or of the Thirty's control, and the nature of the land may also be glanced at. In 350/49 (Androtion, 324F30, Philochorus, 328F155), in the dispute with Megara about the Sacred Orgas, Athens reviewed the boundaries of the Orgas, and, as a result of an oracle, consecrated τὰς ἐσχατιάς, ὅσαι ἦσαν πρὸς τῇ Ὀργάδι (Androtion) or τὰς περὶ τὴν Ὀργάδα (Philochorus). All this is certainly in a frontier area, but so is the Orgas itself, and the ἐσχατιαί may be slightly different. It may be relevant that the consecrated area was probably cultivated before; see *IG* ii^2 204.26, 29, where ἐσχατιά is not used.

There is another strand to the lexicographers. Compare with Bekker, *Anecd.* i. 256.30, quoted above, the first of the definitions in *Schol. ad* Aeschin: ἐσχατιαί, εἰσι τόποι ἔσχατοι τῆς χώρας περατούμενοι ἢ εἰς ὄρη ἢ εἰς θάλασσαν. Is someone confusing ὅροι and ὄρη? The definition 'mountains and sea' is in fact more common. It appears in the Harp.–Phot.–Sud. constellation ἐσχατιά· Δημοσθένης ἐν τῷ πρὸς

Φαίνιππον (XLII.5) τὰ πρὸς τέρμασι τῶν χωρίων ἐσχατιὰς ἔλεγον, οἷς γειτνιᾷ εἴτε ὄρος εἴτε θάλασσα. See also Hesychius: ἐσχατιά · ἄκρα, τὸ ἔσχατον μέρος χωρίου, τὸ συνάπτον τοῖς ὄρεσιν. Pollux, I.221: γεωργικὰ ὀνόματα γῆ, γεωργία, ἀγροικία, ἀγροί, ἐσχατιαί, ἄλση, δρυμοί, δρυμῶνες, ὕλαι, ἕλη, ἴδαι, νάπαι is thinking apparently of a type of land rather than of its situation. The most individual definition is the third in *Schol. ad* Aeschin. not cited by Wilhelm, ἐν τοῖς χωρίοις ἐναπολαμβάνονται ὄρη ἢ ἄκρα κατὰ τὴν Ἀττικήν. παρ' αὐτὰ δὲ ψιλοὶ τόποι εἰσίν, αἳ ἐσχατιαὶ καλοῦνται· τινὲς δὲ ἐργάζονται αὐτούς. For ψιλός, see Pritchett, *Hesperia* 25 (1956), 263.

The facts are more easily accommodated to this second group of definitions. Two properties are described as ἐσχατιαί in the orators. The estate of Phainippos in Demosthenes XLII is notorious (see, most recently, de Ste Croix in *Ancient Society and Institutions . . . V. Ehrenberg*, 109–14). Whatever its area and yield, it is not in dispute that it produced barley, wine and timber, had buildings on it and two threshing-floors. De Ste Croix puts its value at 3 T. 2,000 dr., Davies (*Athenian Propertied Families* 554) at at least 4 T. 3,000 dr. It was not a neglected area. There are suspicions that its contour was distinctly irregular. It was at Kytherros, nowhere near a frontier or the sea. If Kytherros is Spata, it is rather hillier than the plain around. Timarchos (Aeschin. I.97–8) had an ἐσχατιά at Sphettos. This was his own deme and his only property in it that we are told of, but his mother at any rate was more attached to the Alopeke estate and wanted to be buried there. Aeschines describes the Sphettos property as τόπον μὲν πολύν, δεινῶς δ' ἐξηγριωμένον ὑπὸ τούτου. We are not told what it was sold for, but the suggestion is that it was potentially cultivable. || Again, we are nowhere near the frontier or the sea, but we are pretty close to 200 m up.

The texts discussed in this paper give us ἐσχατιαί at Upper Lamptra (away from the sea), Poros, Besa, Kydantidai, Kephale, Phlya, and Aphidna. None of these is very near the sea. The only one which could be described as a frontier deme is Aphidna. Six of its ἐσχατιαί are in the κώμη Hyporeia, which means 'under the mountains', and one, probably also in Hyporeia, is described as ὑπὸ τῷ Φανερῷ, which I take to be a prominent hill. There are fairly satisfactory hills in all these demes, except in Phlya. Prices range from the minimal, two going as a lot for 50 dr. in (25), up to 1,875 dr. for the one in Phlya (49) and 1 T.3,100 dr. for one of those in Upper Lamptra (2).

I only know one other fourth-century text, *Hesperia* 26 (1957), S 2, line 20, where the boundary of a mine of uncertain location, but certainly in hill-country, is partly defined by an ἐσχατιά and a χαράδρα. The restoration in *Hesperia* 5 (1936), no. 9, line 6, is illegitimate; see Pritchett, *CP* 51 (1956), 102 n. 9. ἐσχατιά survives as an Attic land-designation to the time of Hadrian. There are four in *IG* ii^2 2776, one

Πο[——ἐν] μεσο[γείῳ] (line 46), one Ὀρεαδῶν (line 52), which sounds hilly, one at Atene (lines 117–18), which is hilly, one [ἐν Θρ] ιασίῳ (line 197) which is not.

When Timon lost all his money, he retired to painful digging on an ἐσχατιὰ παρὰ τὸν Ὑμηττὸν ἐν τῇ ὑπωρείᾳ (Lucian, *Timon* 6–7, cf. Alciphron, ii. 32 Schepers).

The evidence appears to justify the conclusion that Attic ἐσχατιαί are to be looked for in the neighbourhood of hills. Does it warrant a guess that they were areas which came into cultivation relatively late?

29

The chronology of the Athenian New Style Coinage

Miss M. Thompson[1] has now placed at our disposal a far greater body of evidence for studying the Athenian New Style coinage than was previously available. This is a service which cannot be underestimated. Nevertheless it is my belief that her evidence can be differently interpreted. She begins the coinage in 196/5 and ends it in 88/7. I propose to argue here for a beginning *c.* 164 and an end towards the end of the Roman Republic.[2]

For the purposes of this article I shall assume that her sequence of issues is sound and shall only question her absolute dating. I have indicated doubts about her sequence elsewhere,[3] but do not feel myself qualified to pursue them. The most important single point in her new sequence is the discovery that the coins of ΒΑΣΙΛΕ ΜΙΘΡΑΔΑΤΗΣ–ΑΡΙΣΤΙΩΝ do not belong somewhere in the late two-magistrate issues, but are akin in style to the latest of the three-magistrate issues. I accept this absolutely, but question her deductions from it.

It will be convenient first to argue that my alternative dating is sound and then to deal with the apparent objections. The arguments for the alternative dating fall into four categories: major historical difficulties, the hoard evidence, the Attic prosopography, minor historical difficulties. In order to

* Published in *NC* 7th ser. 2 (1962), 275–300. Miss Thompson replied in the same volume, 301–33. Lewis's chronology has been generally accepted: e.g. Boehringer, *Zur Chronologie mittelhellenistischer Münzserien, 220–160 v. Chr.*, i. 22–39, 200–4; J. K. Davies, *CAH*[2] vii. 1, 277–8.

[1] *The New Style Silver Coinage of Athens.* E. W. Gray and E. S. G. Robinson have helped me greatly with general discussion and particular advice. G. K. Jenkins, C. M. Kraay, and C. C. Vermeule have given other valuable help. In the pages that follow, T = Miss Thompson, L = Lewis.

[2] Once we are free of Miss Thompson's fixed limits, no firm lower limit can be suggested. In view of the scanty nature of the later coinage it is impossible to be confident that we possess every issue or that Athens coined every year.

[3] *CR* n.s. 12 (1962), 291.

avoid misunderstanding, I wish to say that, despite my known interests and the length of the present section on Attic prosopography, this topic has been subsidiary in my thinking and that it was not until the hoard evidence forced on me the belief in a later date that I investigated the prosopography at all closely. The main objections to my dating lie in the Anthedon hoard and in an overstrike. The belief that the Anthedon hoard points || to a beginning in 196 is fundamental to Miss Thompson's dating. I propose to ignore it until after I have developed my alternative chronology.

I MITHRADATES

Having discovered that the ΒΑΣΙΛΕ ΜΙΘΡΑΔΑΤΗΣ–ΑΡΙΣΤΙΩΝ coins belonged higher in the sequence than had been supposed, Miss Thompson thought herself forced to date them *c.* 121 rather than in the natural and universally accepted year 87/6, and found an alternative historical occasion in an otherwise unattested benefaction of Mithradates V. I propose to argue here that the historical arguments for dating Mithradates and Aristion in 87/6[4] are not only strong in themselves but confirmed by the sequence established by her for the nine issues preceding this one. Let us take them year by year.

ΑΡΙΣΤΙΩΝ–ΦΙΛΩΝ Pegasus. 129/8 (T), 96/5 (L)

The appearance of Aristion with a Pontic symbol would certainly seem to point to the tyrant. There are two difficulties about placing this issue as early as 129/8. The first is the chronological difficulty clearly outlined by Miss Thompson herself (pp. 553–4); the second is the fact that, in the present state of our knowledge, the Pegasus symbol would appear not to be generally Pontic, but confined to Mithradates VI and his son (p. 423 n. 2). Neither of these difficulties applies to 96/5. That date does, however, imply that Aristion was in Athens in 96/5 and that the political situation allowed him to use a symbol indicating sympathy with Mithradates. In the state of our knowledge of Athens for this particular year we have no right to find this surprising.[5]

[4] For the political situation and detailed references for this section, see Ferguson, *Hellenistic Athens* 435–52; Wilamowitz, *Athenion und Aristion*.

[5] Pro-Pontic magistrates are also to be assumed for 97/6, for ΝΙΚΗΤΗΣ–ΔΙΟΝΥΣΙΟΣ, tied to ΑΡΙΣΤΙΩΝ–ΦΙΛΩΝ by three die-links, use a gorgon-head, which also has Pontic implications (T., pp. 422–3).

ΑΡΟΠΟΣ–ΜΝΑΣΑΓΟ Winged Agon. 128/7 (T), 95/4 (L)

There is no political point to be made here.

ΞΕΝΟΚΛΗΣ–ΑΡΜΟΞΕΝΟΣ Coiled Serpent. 127/6 (T), 94/3 (L)

These magistrates, in the year of their last issue, were definitely pro-Roman. This is the first year when there are only two magistrates, which may indicate a tightening of control by the pro-Roman party or a lack of candidates for the liturgy. ||

ΝΙΚΟΓΕΝΗΣ–ΚΑΛΛΙΜΑΧΟΣ Hermes or no symbol. 126/5 (T), 93/2 (L)

A year of disturbance, apparently. For the first two months the absence of a third magistrate continues. Though the year began with the use of Hermes as a symbol, this was later deleted on some dies and new dies later in the year bore no symbol. What significance was attached to Hermes it is hard to guess, but disturbance seems more likely in 93/2 than in 126/5.

ΔΗΜΕΑΣ–ΕΡΜΟΚΛΗΣ Head-dress of Isis. 125/4 (T), 92/1 (L)

A quiet year with third magistrates behaving normally and no sign of disturbance.

ΞΕΝΟΚΛΗΣ–ΑΡΜΟΞΕΝΟΣ Dolphin and Trident. 124/3 (T), 91/0 (L)

Xenokles and Harmoxenos return, again without any third magistrate, and coin heavily throughout the year. If this is 91/0, this is the year in which the pro-Roman Medeios returns to the archonship which he had previously held in 101/0, and may be the beginning of the policy of coercion later denounced by Athenion.

ΞΕΝΟΚΛΗΣ–ΑΡΜΟΞΕΝΟΣ Roma. 123/2 (T), 90/89 (L)

Magistrates had held office more than once in earlier years, but 'in this instance precedent was abandoned' (T. p. 396) and Xenokles and Harmoxenos continue to hold office. If this is 90–89, there is a parallel with the archon

Medeios, who did likewise, the first archon to do so for 200 years. Xenokles and Harmoxenos make their political position clear with the Roma symbol, not apparently to the approval of all, since the symbol is defaced on two coins. But their coinage was moderate this year, stopping after the seventh month. Silver supplies may have been short.

ΚΟΙΝΤΟΣ–ΚΛΕΑΣ Roma and Nike. 122/1 (T), 89/8 (L)

On the later dating, though Medeios continues for a third year, Xenokles and Harmoxenos do not. But their successors choose a symbol of even greater loyalty to Rome, as the storm rises in the East. Coinage is moderate again this year. More significantly, it stops after the fourth month in the late autumn of 89 and does not restart. Shortage of silver may be the reason, but, if this is 89/8, it is the year during which Athenion arrived and broke the oligarchy. The continuance of pro-Roman mint-magistrates with a very pro-Roman symbol is not to be expected. ||

ΑΠΕΛΛΙΚΩΝ–ΓΟΡΓΙΑΣ Griffin. 121/0 (T), 88/7 (L)

'The present striking is the last of the three-magistrate emissions and apparently one of the last sizable issues put out by the Athenian mint. From now on the names of only two magistrates appear on the coinage and annual output shows an irregular decline, terminating in the token emissions of the early first century BC' (T. p. 367). I cannot improve on this statement of the significance of the issues of this year, and it would be hard to see a better year for it than 88/7. But in addition the prosopographical evidence is decisive. The griffin proves that Apellikon was Apellikon of Teos, whom we know to have been in exile before this year and influential during it. On the early chronology, he held the mint magistracy as a foreign book-collector in his twenties. This seems less probable.

There is a gap in the coinage in the fifth and sixth months of the year. Does this give the date of his expedition to Delos?

ΒΑΣΙΛΕ ΜΙΘΡΑΔΑΤΗΣ–ΑΡΙΣΤΙΩΝ Star between crescents. c. 121 (T), 87 (L)

In 87 Aristion held Athens as tyrant for Mithradates. The combination of names, the Pontic symbol, and the gold coins of crisis have always been held

to date the issue. The issue in previous metals was small and is not known to have continued after the sixth month. Athens will have had other things to think about as 87/6 wore on. I can only side with those who have thought 87 the natural, indeed, the only possible date. Miss Thompson's view of a benefaction issue of Mithradates V has no attractions by comparison.

It will be seen that this issue does not stand alone. Once it is given its proper date, the preceding issues fall easily into place in the known historical framework. It is hard to believe that these tensions between three-magistrate and two-magistrate issues, between Roman and Pontic symbols, between pro-Roman and pro-Pontic magistrates, belong to the period between 129 and 121. Their natural placing is thirty-three years later.

II THE HOARD-EVIDENCE

This thirty-three-year gap does not stand alone. I propose next to examine the hoard-evidence, hoard by hoard, in Miss Thompson's order, and to show that here too the hypothesis that her chronology is 30–35 years too high gives a clearer picture. ||

Anthedon hoard. This hoard is the foundation of Miss Thompson's chronology. It is discussed below (Section v).

Tel Ahmar. Only five tetradrachms are certainly known from this hoard, but all Athenian coins in it clearly belonged to the early issues with monograms or abbreviated names. This hoard, like Kessab below, contains 'New Style coins of comparatively early date in association with late Seleucid issues. In the present hoard the gap between the last New Style coin of which we have record and the earliest Seleucid is thirty-five years while the hoard as a whole must have been buried early in the first century BC, about eighty years after the latest New Style striking.' The recorded Athenian coins all show considerable wear. Miss Thompson's deduction is that 'well before the middle of the second century BC Athenian tetradrachms had ceased to move eastward in quantity'. Another possible deduction might be that her date for the Athenian coins is too high by thirty-five years or possibly a little more.

Salonika. 300 Athenian tetradrachms from six issues only, dated by Miss Thompson to 170–164. No judgement on the absolute chronology

is possible, but it should perhaps be noted that these are strange years for Athenian coins to be reaching Macedonia in quantity.[6]

Kessab. This hoard contained late Seleucid issues from Demetrius I to Antiochus IX, the latest being dated to 113/12 BC. It also contained forty Athenian pieces, of which the latest, dated by Miss Thompson to 164/3 and 163/2, are in a splendid state of preservation. Like the Tel Ahmar hoard above it therefore appears to combine late Seleucid and early Athenian issues. Miss Thompson considers two possibilities: (*a*) that the Athenian pieces represent a separate hoard merged at a later date with Seleucid material; (*b*) that this hoard is a continuing accumulation with the Demetrius tetradrachms picking up where the New Style issues stop. She admits that, in this latter case, it is difficult to explain the absence of earlier Seleucid issues. A third possibility suggests itself, that the latest Athenian issues in their fine state are not to be dated *c.* 163, but *c.* 130, seventeen years or so only before the burial of the hoard. If all the Athenian issues are scaled down similarly they become almost exactly contemporary with the Seleucid material. ||

Naxos. This hoard has Athenian coins, of which the latest is dated by Miss Thompson to 159/8. The non-Athenian material is not closely datable. The possibility of piracy does not allow one to choose between a burial-date for the hoard in the 150s or one in the 120s.

Attic hoard. This hoard, mainly of drachmae, has as its latest coin a drachma dated 155/4, with all coins dated to the 160s in good condition. No useful comment can be made about the burial-date of this hoard.

Delos Γ. 249 tetradrachms, Attic only. 'In all probability the hoard was buried *c.* 137 BC.' The burial could have been caused by the slave-revolt of 133, and this is perhaps more probable than invading pirates *c.* 104. If this hoard has an historical explanation, it is in favour of the high chronology.

Delos K5. 39 Attic to 131/0 and a Pergamene piece of roughly the same date as the latest piece. All specimens have circulated.

Delos B. A small-change deposit, probably a savings accumulation, ending in 130/29 (T).

Delos ΛH. A very large hoard, only Attic, ending in 130/29 (T).

[6] They could of course all have reached Macedonia together after the end of the war. It is only gradual accumulation in Macedonia itself which is improbable.

Miss Thompson argues that these three hoards are connected with the slave-revolt of Delos, which she dates in 129 BC against Ferguson's date of 130[7] and Lauffer's date of 133.[8] There is little information about the condition of the latest coins in the hoards, except that in Ks all specimens had circulated. If it is legitimate to date the revolt by the hoards, her picture is plausible and considerably better than any case that can be put up for a late chronology. I know of no indication of trouble in Delos in, say, 98.

Zarova. A very large hoard, only Attic, ending in 127/6 (T). There is nothing very much to choose between this year and 94/3 (L) as a burial date.

Halmyros. An even larger hoard, only Attic, ending in five coins of 124/3 (T). Here the historical probabilities begin to swing the other way. No historical argument for 124/3 could compete in attraction with the likelihood that a large hoard would be buried in Thessaly shortly after 91/0 (L). It will be recalled that 90/89 (L) and 89/8 (L) were years of moderate minting, and their products might well not have reached Thessaly before the Roman armies. ||

Carystos II. Attic only, ending in 123/2 (T), 90/89 (L), with coins very good or FDC. Again the historical arguments for the later date are overwhelming.

Ontario. No provenance, ending in 136/5 (T) with a single coin of 121/0 (T), that is *c.* 104 (L), with a single coin of 88/7 (L). This hoard is too peculiar to argue from.

Carystos I. The Attic ended in 121/0 (T), 88/7 (L), the latest coins being in good condition. There were Carystian coins, undatable, but (more significantly) three tetradrachms inscribed ΑΘΕ Ο ΔΕΜΟΣ. The weight of opinion has always been in favour of dating this last issue to 88–86, and Miss Thompson herself firmly agrees (pp. 444–9). There is therefore a strong argument for dating the burial of this hoard to 87/6. The absence of any Athenian coins later than 121/0 and the good condition of coins of the 120s is therefore a puzzle which Miss Thompson does not directly explain. The simplest explanation is that the Athenian coins end in 88/7, not in 121/0.

Piraeus. The Attic end in 121/0 (T), 88/7 (L). The last of these coins are in good condition, but markedly inferior to two coins of Mithradates VI, not precisely datable. The late chronology makes impeccable historical sense, a

[7] Ferguson, *Hellenistic Athens* 379.

[8] Lauffer, *Die Bergwerkssklaven von Laureion* 233–6.

burial before the sack of Piraeus in 86, including two coins brought by the partisans of Mithradates. The early chronology offers no attractions.

Abruzzi. This hoard is plausibly assigned to loot of a Sullan soldier from Athens. The Attic end in coins of fine to FDC condition of 121/0 (T), 88/7 (L). There are also 9 ΜΗ–ΜΗ tetradrachms dated by Miss Thompson, convincingly, *c.* 86/5; 4 coins of Mithradates of 90/89, also in excellent condition; and about 200 Roman denarii of the time of Sulla. The late chronology fits perfectly. The soldier helped himself to what he could find, and not unnaturally the best coins he found were issued immediately before the siege. The early chronology once again has a fearsome thirty-three-year gap. Why, in the circumstances, should there be no single Athenian coin dated by Miss Thompson after 121/0? She offers two explanations: (p. 506) the regular Athenian issues formed a small hoard which the soldier had come upon and pocketed. She then realises (p. 543) that this explanation will not fit the similar gap in the Piraeus hoard (above) and Dipylon hoard (below), and says that there was little or no new money available in the Athens area after 121/0. This explanation is || supplemented (p. 542 n. 2; 714) with the suggestion that some of the money coined in the 120s was actually put into circulation later than the date of striking. I find this very forced and, as far as I know, it is not supported by the only kind of evidence which would support it, namely, the finding in the same hoard of coins of the same issue of widely differing states of wear. I think it can be claimed that the evidence of the Abruzzi hoard is very strongly in favour of a low chronology.

Dipylon. The same pattern is here repeated. The Attic coins in the hoard this time end with 'stempelfrisch' coins of ΒΑΣΙΛΕ ΜΙΘΡΑΔΑΤΗΣ–ΑΡΙΣΤΙΩΝ *c.* 121 (T), 87/6 (L). There are also four Mithradatic tetradrachms probably to be dated in the early 80s. Miss Thompson's explanation (p. 509) that the owner had not been able to put aside coins between 121 and 87, but acquired some Mithradatic coins just before burying his hoard in late winter 87/6, supplemented by the suggestion discussed above, is in defiance of all historical probability. If any further proof were needed that the ΒΑΣΙΛΕ ΜΙΘΡΑΔΑΤΗΣ–ΑΡΙΣΤΙΩΝ issue belongs to 87/6, the Dipylon hoard provides it.

Anatolia. The material which certainly belongs to this hoard is all Attic and ends in 120/19 (T), that is, it contains one coin of ΜΝΑΣΕΑΣ–ΝΕΣΤΩΡ. If Miss Thompson's sequence is sound, the low chronology would suggest that this issue belongs after Sulla's sack of Athens. The chronological

importance of this hoard depends on whether the Cappadocian coins, including coins of Ariarathes IX (99–87) and one of Ariobarzanes I, i.e. after 95, said by the dealer to belong to it, do so belong. If they do, there is once again a gap, on Miss Thompson's chronology, between Attic coins in excellent condition and the burial date of the hoard. The low chronology presents no difficulties.

Cretan II. The Attic end with a splendid coin of ΑΡΧΙΤΙΜΟΣ–ΔΗΜΗΤΡΙ. 117/16 (T), (?) *c.* 80 (L). Among the rest, there are three extremely fine dated pieces, two of Nikomedes IV, 92/1 and 90/89, and one of Mithradates Eupator, 76/5. There is again, on the high chronology, a large gap. Miss Thompson assumes that the Attic pieces were put aside long before 75. The low chronology presents no problems.

Delos ΞΘ. All Attic, ending in 112/11 (T). The latest coins had not circulated for many years before burial. Miss Thompson suggests no || burial dates. On the low chronology, ΣΩΤΑΔΗΣ–ΘΕΜΙΣΤΟΚΛΗΣ must come down at least to the mid-seventies. Perhaps the burial is to be attributed to the sack of Delos in 69.

Delos A. Again the Attic end with ΣΩΤΑΔΗΣ–ΘΕΜΙΣΤΟΚΛΗΣ, this time FDC. The same remarks about burial date apply. But in this case there is a new element, the appearance of two ΜΗ–ΜΗ drachmas, dated by Miss Thompson to 86/5. Even if these were in good condition there would be, on the high chronology, a gap to explain between 112/11 and 86/5. Miss Thompson gives us no information about their condition and no photographs, but they might repay investigation, since, if they show any considerable signs of wear, they would indicate that there was New Style coinage even after the sack of Athens. I have now looked for them, unsuccessfully.

Cretan I. Attic only, ending 92/1 (T), (?) 55–45 (L). Nothing useful emerges. It should be noted that the hoard contains a coin of ΔΗΜΕΑΣ–ΚΑΛΛΙΚΡΑΤΙΔΗΣ. This issue will be discussed later (Section VI).

Hierapytna. The Attic end 94/3 (T), (?) 55–45 (L). None of the two-magistrate issues were at all badly worn; Kambanis described some of them as very fresh. The accompanying cistophori and Roman Republican denarii enabled Raven to fix the burial date of the hoard between 44 and 42. On the high chronology, the gap between Athenian coins in good condition and this burial date is inexplicable. It is certainly not satisfactorily explained by Miss Thompson (p. 542).

The remaining hoards can be more briefly disposed of. For some the evidence is totally inadequate.

Oreos. Last known coin 130/29 (T), 97/6 (L). All Attic.

Salamis. Last known coin 134/3 (T), 101/0 (L). All Attic. Second slave revolt?

Delos. (Noe 319) No comment.

(Noe 309) No comment.

(Noe 303 and Noe 304) Both said to end with ΞΕΝΟΚΛΗΣ–ΑΡΜΟΞΕΝΟΣ issues, but note Miss Thompson's warning (p. 519 n. 1). Even allowing for this a burial date *c.* 87, as is demanded by the low chronology, would seem to fit well.

Noe 315 (2) = Delos IΓ ends 124/3 (T), 91/0 (L) The same applies. ||

JIAN 1911, 76, ends 123/2 (T), 90–89 (L), with an excellent drachma. The same applies.

Noe 312 = Delos IΔ The Attic end 127/6 (T), 94/3 (L), and comprise six coins. The other seven coins in the hoard are Mithradates coins of 93/2. The high chronology involves the usual gap, but, as the hoard (including some of the Mithradates coins) is in bad condition, no firm argument can be derived from it.

Noe 311. The straightforward Attic end with single coins of 129/8 and 128/7 (T), 96/5 and 95/4 (L). It will be seen from Miss Thompson's plates that they are in quite good condition. The hoard also contains five of the ΜΗ–ΜΗ issues dated by Miss Thompson 86/5 and a cistophorus of Ephesus dated 89. On the high chronology, again a very considerable gap.

Noe 313. Attic end 116/15 (T), *c.* 80 (L), and there is also a ΜΗ–ΜΗ drachma. A gap, on the high chronology; but the condition of the coins is such that no inference can be fairly drawn.

Before we proceed, it may be as well to draw attention to the state of the Delos evidence as a whole.

> The record of the Delos material as a whole – large hoards, small hoards and chance finds – is absolutely consistent and somewhat puzzling. While there is a limited amount of Mithradatic and Sullan silver, there is nothing from the regular Athenian mint after 112/11. . . . Whatever this means, it clearly can have nothing to do with Archelaus and the devastation of the island. The argument that the Athenian coinage went on after Sulla gains nothing from the evidence of the Delos coins. [T, pp. 412–13]

This is certainly an argument against those who believed that there was no life on Delos after 87, but as an historical picture in itself it is flatly

incredible. Relations between Athens and Delos continued to be lively after 112; the slightest knowledge of the inscriptions will show that. That these relations are not reflected by a single coin before the ΗΗ–ΗΗ issues seems highly improbable. The low chronology accommodates the evidence comfortably, with at least six hoards reflecting the events of 87, and a decent amount of (admittedly reduced) activity thereafter until the late 70s. Only in the three hoards buried *c.* 129 or *c.* 98 does the high chronology fit the historical evidence for Delos better.

Of the hoards described by Miss Thompson as being of minor significance (pp. 521–3) only two call for comment: ||

Beliza. In the coins acquired by the Sofia museum there is a gap, on the high chronology, between 117/16 and 86/5.

Greece? (Noe 462). The 14 Athenian pieces ended with ΞΕΝΟΚΛΗΣ–ΑΡΜΟΞΕΝΟΣ (127/6 or later (T); note again the warning, p. 519 n. 1), but there were 20 ΗΗ–ΗΗ tetradrachms and about 50 Roman denarii, dated by both Sydenham and Pink to the early 80s. No information about condition is given. Prima facie the high chronology creates the usual gap.

Of the hoards with a single New Style coin (p. 523), only two record the condition of the coin satisfactorily:

Babylon. The hoard was buried *c.* 150. The Attic coin is dated by Miss Thompson to 189/8. Her plate shows it to be in first-class condition, except that it has the deposit usual in this hoard. I return to this hoard in Section v.

1934–5. Latest dated coins, Mithradatic tetradrachms 75/4. The Attic coin is 118/17 (T) and can be seen from her plate to be fine to FDC.

No useful conclusion can be drawn from the coins in the Hunterian Collection (p. 524), but it should perhaps be noted that the Istanbul group (pp. 524–5), thought by Miss Thompson to be probably associated, has a gap on the high chronology between 120/19 and a Ο ΔΕΜΟΣ coin dated by Miss Thompson in 86.

There is one more hoard, in Boston, from Chesmé (p. 448 n. 1). The last Attic is 124/3 (T); there is a Mithradates tetradrachm of 89/8 and two Ο ΔΕΜΟΣ pieces. Vermeule tells me that the whole hoard was very much corroded, that the Attic was 'very slightly worn', the Ο ΔΕΜΟΣ coins not so much so. There may be a gap, but this hoard does not seem to prove anything by itself.

I have deliberately, in this section, set aside the Anthedon hoard, which Miss Thompson uses to prove that the series begins in 196/5, and reserve it

for fuller discussion in Section v. With this evidence set on one side, I think it can be claimed that no one would suspect from the rest of the hoard evidence that the series starts so early. In every hoard where Attic New Style is found with material otherwise datable, there is a clear gap, on the high chronology, between the latest Athenian coins, often demonstrably in fine condition, and the burial date of the hoard. Problem after problem arises, demanding complicated solutions. On the low chronology these problems do not || exist. Where the hoards have only Athenian coins no cogent argument can work either way. For some of these hoards the low chronology has enabled me to suggest burial dates which seem more satisfactory historically, but it must be admitted that, with the Delos hoards, Kς, B, and ΛH, the balance of probabilities is with the high chronology if an historical solution has to be found.

III THE ATTIC PROSOPOGRAPHY

By itself the prosopography can prove nothing. Unless and until the mint-magistrates are shown to hold a known office in the state,[9] the Attic prosopography will not provide an exact date for a single coin, and the history of the question does not suggest that it has been very successful in providing approximate dates. What it might be able to do in theory is to provide approximate dates for groups of coins, but even here there are two main pitfalls.

1. The investigator has to work, for the most part, from single names, although even identifications made on the basis of name *and* demotic often prove fallacious. This difficulty is counterbalanced to some extent by two considerations. Firstly, the annual symbols ought to mean something in relation to an individual, and sometimes we may be able to make a guess at the individual from the symbol. However, there are few symbols of which it would not be possible to argue that they were equally applicable to more than one generation of the same family. Secondly, pairs of magistrates often have names which we think we recognise as belonging to the same family. This is an inherently reasonable pattern, and identifications of this kind are attractive. However, I would judge that it is not sound method to go beyond

[9] I would agree with Miss Thompson that such a demonstration is highly improbable for the bulk of the coinage, but I would reserve judgement on the issues I hold to be post-Sullan. ΔIOKΛHΣ TO ΔEY and TO TPI sound a little too precise for a liturgy.

the pair of annual magistrates in such identifications. When it comes to introducing third magistrates into family relationships with an annual magistrate, another third magistrate of the same year or even of a different year, the possibility of coincidence is too great to make the identification profitable, unless the names are very uncommon. I would also confine such family pairing to combinations of names directly attested in the second and first centuries; to go as far back as the fourth century is, I think, dangerous.

2. The investigator might be deluded by the nature of his evidence. He might think that he had an even coverage of information over all || the relevant period, whereas in fact there might be a shortage of the names of the right kind in one portion of it. This would lead him to claim an identification as highly probable, although fuller information would provide alternative possibilities. I think this point may be valid here; in between *IG* ii^2 2332 (183/2) and *IG* ii^2 2336 (103–96) there is nothing quite like them as evidence for a cross-section of the propertied classes, and we rely on a combination of more scattered evidence to fill the gap.

Even with these qualifications, I think it is worth considering some prosopographical points here. In her section on Magistrates, Miss Thompson, inevitably and, I think, in principle rightly, assumed that her dates were right and proceeded from them to identifications. She does discuss some identifications which were made earlier, but, since Kirchner and Sundwall assumed an earlier start as well as a later end for the coinage, some identifications which seem to me possible or likely do not appear. It would take too much space here, without essentially advancing our inquiry, to discuss simple 'tit-for-tat' identifications, to give alternatives demanded by the low chronology to replace those suggested by Miss Thompson for the high chronology. I hope that the reader will here simply accept that there are alternatives to the vast majority of Miss Thompson's identifications. There still remains a small residue where, on the present state of our evidence, I cannot yet suggest a plausible alternative: e.g. ΑΡΙΣΤΑΙ, ΕΙΚΑΔΙΟΣ (if the interpretation of the monogram is correct), ΛΑΙΝΗΣ, ΛΕΟΝΤΟΜΕΝΗΣ. But it would appear that these are more than outweighed by a considerable number of cases where the low chronology gives an identification and the high chronology does not. I give a selection of these; the Arabic number in brackets refers to the position as magistrate in the year.[10]

[10] *IG* = *Inscriptiones Graecae*; *ID* = *Inscriptions de Délos*; *FD* = *Fouilles de Delphes*; *PA* = *Prosopographia Attica*.

ΑΡΙΣΤΟΝΟΥΣ (3) 121/0 (T), 88/7 (L). *PA* 2041, Priest 101/0.

ΑΣΚΛΑΠΩΝ (3) 151/0 (T), *c.* 120 (L); (3) 148/7 (T), *c.* 117 (L); (3) 145/4 (T), *c.* 112 (L); (3) 143/2 (T), *c.* 110 (L). I think Miss Thompson here misses the point that there is no reason to assume relationship between Asklapon and Timokrates, other than that derived from Kirchner's erroneous restoration of *FD* iii. 2 15. Timokrates is a common name, and is probably not relevant, even though a man or men of this name twice served as third magistrate in the same year as Asklapon. The easy identification || of Asklapon is as father of the Thesmothetes of 97/6 (*IG* ii² 2336. 253 with Kirchner's notes and *Hesperia* 21 (1952), 362 for a grandfather), but see *Hesperia* Supp. 1 (1937), no. 84 = *Agora* xv 225. 60 for the appearance of the name in another family.

ΛΑΜΙΟΣ (3) 146/5 (T), *c.* 114 (L); (3) 142/1 (T), *c.* 110 (L). *PA* 8986, Secretary of the Council, 112/11.

ΜΕΝΟΙ (3) 149/8 (T), *c.* 117 (L). *PA* 10058, Archon 117/16.

ΝΙΚΟΝΟ (3) 141/0 (T), *c.* 109 (L). *PA* 10980, Thesmothetes 97/6.

These will not prove much by themselves. More serious problems are raised by difficulties about the age of nearly certain identifications. Aristion has been fully dealt with in Section 1. Apellikon I have also dealt with in part, and only point out that the low chronology helps further, in that it inevitably dissociates the two Apellikon issues and leaves the second, dated by Miss Thompson in 94/3, for a descendant, father of the early Augustan archon Apolexis (*Hesperia* Supp. 1 (1937), p. 191 n. 1; and *Hesperia* 26 (1957), 251). Boukattes is to be identified with a knight of 128/7; if his mint-magistracy was in 156/5, as the high chronology demands, he would have been a somewhat mature knight.

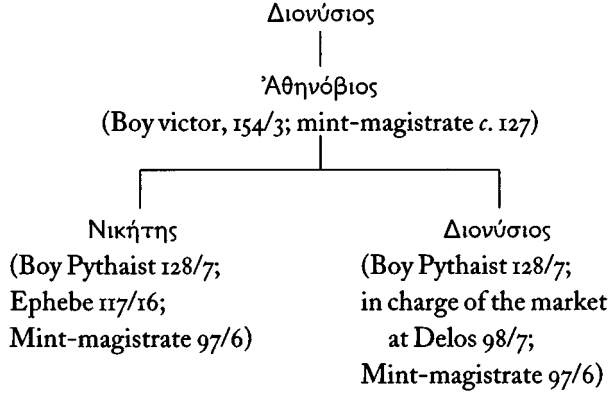
Miss Thompson (pp. 410–11) rejects the case put up by Kirchner (*ZfN* 21 (1898), 74–105) for supposing the bulk of the late two-magistrate issues to be post-Sullan. She has little difficulty in showing that some reasonable identifications can be made with pre-Sullan figures. But it seems to me that individual identifications do not attack the strength of the case. This strength is cumulative. We have fifty or sixty names which Miss Thompson wishes to distribute over the years 120–87. This is a period for which our epigraphic evidence is copious, and yet the percentage of plausible identifications which can be suggested is no better than fair, and many people whom we know from *IG* ii² 2336 to have been wealthy and prominent will somehow have evaded mint duties. Kirchner and I wish to make them post-Sullan.

This is a period for which our epigraphic evidence is extremely poor. Yet nearly three-quarters of the names preserved on the coins can be readily associated with archons and others known to be of the highest importance in the period.

So much for arguments from single names. If we argue from pairs, it seems to me possible to arrive at more solid ground. I now consider cases where it can be argued that the high chronology introduces a distorting factor into certain family trees by forcing the introduction || of new generations where there is no epigraphical evidence for them, whereas the low chronology does no violence to the other evidence.

A ΑΘΗΝΟΒΙ *(3): 159/8 (T), c. 127 (L)*
ΝΙΚΗΤΗΣ *(1)–*ΔΙΟΝΥΣΙΟΣ *(2): 130/29 (T), 97/6 (L)*

Even ignoring the appearance of ΝΙΚ and ΔΙΟΝ as third magistrates in 132/1, Miss Thompson's association of these names as members of a family from Eupyridai is attractive. But she does not conceal that her dating involves the assumption that none of these three people on the coins is identical with those known from other sources. The stemma will have to be reconstructed, and I do not see how one would begin to do it. I add two points to her discussion. Daux[11] has argued strongly that the Pythaists of *FD* iii.2 12 are boys. Νικήτης Ἀθηνοβίου Εὐπυρίδης was an ephebe in 117/16 (*IG* ii² 1009.65). On the low chronology, there are no problems.



[11] *Delphes au II^e et au I^er siècle* 716 with n. 2.

B ΑΛΚΕΤΗΣ (2): 157/6 (T), c. 125 (L)
ΑΛΚΕΤΗΣ (1)–ΕΥΑΓΙΩΝ (2): 106/5 (T), c. 72 (L)

There is confusion here, largely caused by Kirchner. These names occur in two demes, and, although there is a strong probability of relationship, nothing is gained by amalgamating the stemmata and assuming that the family suddenly changed its deme. There is evidence for the following:

(A) in Kothokidai.

Alketes (I) only known from his son:

Euagion, ambassador from Delos to Athens 144/3 (*ID* 1507); *epimeletes* of Delos 135/4 (*ID* 1750); on a mysterious base at Athens *c.* 140 (*IG* ii² 2445.14); contributes to a theatre-repair fund *c.* 150 (*IG* ii² 2334.67) on behalf of his wife Sosikrateia,[12] his daughter Philia and his son.

Alketes (II) *epimeletes* (?) *c.* 130 (*IG* ii² 1939.57). The wife, daughter, and son reappear in *IG* ii² 4032.

(B) There is no reference in either *IG* ii² 2334 or 4032 to another son Euagion, as is required by Kirchner's stemma (last version in *IG* ii² 4032). Let us take the Perithoidai family separately. Here we have

Alketes son of Euagion in a list dated by Kirchner *c.* 95 (*IG* ii² 2460.1);

Alketes son of Alketes polemarch in the Augustan period (*IG* ii² 1722.1).

Since *IG* ii² 2460 gives no information about ages, we cannot make any useful deduction about how the families fit together by adoption, if they do, nor about the relationship of the Augustan polemarch to Ἀλκέτης Εὐαγίωνος Περιθοίδης. To pass to the coins, it is clear that the first issue could equally well be associated with Alketes (I) or Alketes (II) of Kothokidai. The second issue is associated by Miss Thompson with brothers, Alketes (II) of Kothokidai and Euagion of Perithoidai, but there is no clear evidence that such brothers existed. On the low chronology, the most likely assumption is that we have a father and son, the directly attested Ἀλκέτης Εὐαγίωνος Περιθοίδης and an unknown son, perhaps grandfather of the polemarch.

C ΕΥΜΑΡΕΙ (3): 155/4 (T), c. 123 (L)
ΕΥΜΑ (3); 152/1 (T), c. 120 (L)
ΕΥΜΑΡΕΙΔΗΣ (1)–ΑΛΚΙΔΑΜ (2): 145/4 (T), c. 113 (L)

The evidence is mostly unambiguous. Eumareides son of Euphanes was an ephebe in 138/7 (*FD* iii.2 23; cf. Daux, *Delphes au II^e et au I^er siècle*, 540 n 1); he

[12] Σωκρατε[ίας] in *IG* ii² is a misprint.

and his brother Alkidamos were knights in the Pythais of 128/7 (*FD* iii.2 27) and Alkidamos carried the tripod in that year (*FD* iii.2 33). The only really doubtful point is whether the Eumareides son of Euphanes of *IG* ii² 2980 belongs to the beginning of the century as Kirchner thought, in which case he would be the grandfather of our Eumareides, or to 138/7 and identical with him, as Sundwall thought; I do not see how he could be a cousin, as Miss Thompson thinks. Miss Thompson identifies the magistrates with the knights of 128/7, but to ignore the evidence of *FD* iii.2 23 || about Eumareides' age is arbitrary, creates three Eumareides where there may be only one, and produces an impossible stemma. The low chronology creates no problems, and allows the possibility that Alkidamos' replacement early in his year was due to death.

D ΟΙΝΟΦΙΛΟΣ (2): 116/15 (T), c. *81 (L)*
ΑΜΦΙΑΣ (1)–ΟΙΝΟΦΙΛΟΣ (2): 115/14 (T), c. *80 (L)*

Οἰνόφιλος Ἀμφίου was βασιλεύς in 88/7 (*IG* ii² 1714; cf. *Hesperia* 3 (1934), 144–6 for the date); there is no particular reason to doubt that his father could have survived to hold the magistracy with him. In fact, the most economical hypothesis does require Oinophilos to be on the young side in 88/7. The alternative is to have a second man of the same name and patronymic rather close to him in date (*IG* ii² 1755; *Hesperia* Supp. 1 (1937), 170 for the date). The Eukles of *IG* ii² 3151 will be his son. The most serious cause for doubt is that he held office in the revolutionary year 88/7; he seems to have made his peace with the reaction.

E ΕΠΙΣΤΡΑ (3): 168/7 (T), c. *136 (L)*
ΑΜΦΙΚΡΑΤΙ (3): 166/5 (T), c. *134 (L)*
ΑΜΦΙΚ (3): 162/1 (T), c. *130 (L)*
ΑΜΦΙΚΡΑΤΗΣ (1)–ΕΠΙΣΤΡΑΤΟΣ (2): 133/2 (T), c. *101 (L)*

It should perhaps be pointed out that ΑΜΦΙΚΡΑΤΙ is Ἀμφικρατίδης not Ἀμφικράτης and that ΑΜΦΙΚ is not certainly either. The epigraphic evidence is straightforward. Amphikrates son of Epistratos of Perithoidai was in charge of the great collection of *aparchai* which began in 103/2 (*IG* ii² 2336.2; *HSCP* 51 (1940), 116 for the text). He carried the tripod to Delphi in the Pythais of 97/6 (*FD* iii.2 32). He had been ὁ ἐπὶ τὰς ἀπαρχάς in the Pythais

of 106/5 (*FD* iii.2 13). His brother seems to have been less distinguished, and is only known as secretary to the ephebes in 102/1 (*IG* ii² 1028. 54, 147). The low chronology involves no disturbance to these facts, beyond possibly giving the elder Epistratos a brother Amphikratides, and puts Amphikrates' mint-magistracy in his main period of activity. Miss Thompson's stemma gives us three pairs of brothers, identically named, in subsequent generations. The low chronology seems simpler.

F ΘΕΜΙΣΤΟΚΛΗΣ

This is a straightforward problem, and the evidence need not be set out in full. Coins with the name of Themistocles fall into two groups. The first contains four issues, dated 165–148 (T), *c.* 133–115 (L). In the last of these issues Themistocles is the || first magistrate, and the galley symbol identifies the family. The second has two issues Themistocles as (2) in 112/11 (T), *c.* 78 (L), Theophrastos as (1) and Themistocles as (2) in 109/8 (T), *c.* 75 (L). Now the direct male line of this family is fully and certainly known; the stemma is most conveniently to be found in the notes to *IG* ii² 3510. The first Themistocles in it went to Delphi as Πυθαϊστης ἐκ Κηρύκων in 106/5. Miss Thompson identifies the magistrates of the second group with him and his father Theophrastos (1). But this leaves the first group for an unknown. It will be seen from the stemma that the most economical arrangement is:

(perhaps) Theophrastos (I), mint-magistrate three times, *c.* 137–130;
Themistocles (I), mint-magistrate four times, *c.* 133–116;
Theophrastos (II), mint-magistrate, *c.* 75;
Themistocles (II), mint-magistrate twice, *c.* 78–75.

This involves no unknown person. It does involve raising the *floruit* dates by about fifteen years, but they rest on nothing except the one dated appearance of Themistocles (I). As he does not reappear in the Pythais of 97/6 he may well have been elderly in 106/5.

G ΠΟΛΕΜΩΝ(1)–ΠΑΤΡΩΝ(3): 157/6 (T), c. 125 (L)

If these names are connected, we should note that Kirchner was dating these brothers by the thesmothetes of *IG* ii² 1714. This inscription is to be dated to 88/7 (*Hesperia* 3 (1934), 144–6), and Patron son of Polemon there might even be son of the Polemon of the coins.

H ΤΙΜΟΣΤΡΑΤΟΣ(1)–ΠΟΣΗΣ(2): 134/3 (T), c. 101 (L)

Their symbol is Dionysos with mask, which makes it clear that we are dealing with the dramatic family of Phaleron. Let us reconsider the evidence for this family.

1 Timostratos (I), competes at Dionysia, 189/8, 184/3 (*IG* ii² 2323.141, 155); contributes to a fund, 183/2 (*IG* ii² 2332.206).

2 Ariston, son of Timostratos, comic poet at Samos, date uncertain (Michel 901. 10).

3 Timostratos, son of Ariston, boy Pythaist, 128/7 (*FD* iii.2 12); nobleman in the cumulative catalogue *IG* ii² 2452.42.

4 Poses, son of Ariston, comic poet at Tanagra *c.* 100 (*IG* vii 540; Ἀρχ. Ἐφ. (1956), 44–5);[13] gymnasiarch at Delos *c.* 100 (*ID* 1928); thesmothetes 88/7 (*IG* ii² 1714; *Hesperia* 3 (1934), 144–6). ||

5 Ariston, son of Poses, councillor *c.* 50 (*Hesperia* Supp. 1 (1937), no. 102, line 49); comic poet at Oropos, a little earlier (*IG* vii 416).

All the new facts seem to me to support Kirchner's stemma, and this stemma unequivocally supports the low chronology. The mint-magistrates are nos. 3 and 4 above. Miss Thompson's stemma creates three new persons who have no supporting evidence.

One similar case, however, might reasonably be taken to point the other way.

I ΙΚΕΣΙΟΣ(1)–ΑΣΚΛΗΠΙΑΔΗΣ(2): 135/4 (T), c. 103 (L)

The evidence for the date when Asklepiades son of Hikesios was *hieromnemon* leads to no clear conclusion.[14] A year *c.* 125 certainly suits very well. However, his appearance at Tanagra now seems to be close to 100,[15] which makes more likely Kirchner's guess that he is the Asklepiades of Halai who was priest of Dionysos in 101/0 (*IG* ii² 2336.123). If Hikesios is his father, this would argue for the high chronology. But no one could say that a date *c.* 125

[13] Christou here adds other members to the family, without sufficient justification, as far as I can see.

[14] Daux, *Delphes au IIe et au Ier siècle* 622–3. [15] Ἀρχ. Ἐφ. (1956), 46–7.

as *hieromnemon* was certain. If it were, I should be inclined to make Hikesios an elder brother.

To put the prosopographical evidence cautiously, it would seem reasonable to say that here too the high chronology involves complications which do not exist on the low chronology.

IV MINOR HISTORICAL ARGUMENTS

None of the three points which follow is conclusive, but they seem to add some force to the case for the low chronology.

1. Robert, in a classic chapter,[16] traced the history of the phrases δραχμὴ στεφανηφόρος, δραχμὴ (τοῦ) στεφανηφόρου and showed that they simply referred to Attic New Style coins. The phrase first appears at Delos *c.* 157, and soon after that date Athenian officials at Delos were handling side by side the new coins and the old, the γλαυκοφόρα. It seems to me that Robert was right to claim that these facts had chronological implications, that precision of designation was more likely near the beginning of the coinage and that the lower the date of the beginning, the more easily the texts could be explained. He consequently welcomed Bellinger's proposal to lower the start of the coinage to *c.* 180. The even lower dating proposed here suits the texts better still. But there is an obvious reply to this argument. We are || near the beginning of Athenian control in Delos. The coins are new to Delos, not new in themselves. I do not press the evidence, but find it suggestive.

2. Miss Thompson (pp. 635–6) finds a marked break in the sources of Attic silver in her Late Period, which she begins in 131. Her case for a disturbance in Laurion silver-production is very strong. She suggests that this disturbance is connected with the first slave-revolt there, to which she gives far-reaching effects. But this leaves the second slave-revolt of 102–100 little to do, and such literary evidence as we possess suggests that it was a far more substantial affair.[17] The low chronology allows the break in the silver to be caused by the second slave-revolt.

3. Miss Thompson nowhere mentions the remarkable Amphictyonic Decree,[18] well discussed by Daux,[19] which gives the Athenian tetradrachm a privileged place among Greek currencies. It is surely clear that this was done

[16] *Études de numismatique grecque* 105–35. [17] Lauffer, *Die Bergwerksklaven* 227–40.
[18] *SIG*[3] 729. [19] *Delphes au II*[e] *et au I*[er] *siècle* 387–91.

at Athens's request. Its date is unfortunately uncertain. Daux implies that the extreme outside limits are 124–100, more probably 121–100.[20] On Miss Thompson's chronology this would imply that the decree was passed at least at a time when the Athenian mint did not know where its next supply of silver was coming from, more probably at a time when it had ceased to coin in quantity at all. This does not seem likely.

V THE ANTHEDON HOARD

There is nothing in the evidence we have discussed so far which would make anyone suspect that the high chronology was a possibility. Our procedure has been roughly the same as Bellinger's in 1949.[21] We have taken Mithradates–Aristion as a fixed point, greatly encouraged by the sequence now built up in the years before it, and have worked back to a starting date somewhere in the 160s. Such a chronology has given a convincing picture when applied to all the other evidence, principally the hoard-evidence. Miss Thompson[22] has, however, preferred another peg, the Anthedon hoard, from which she works down, instead of working up. The evidence provided by this hoard must now be discussed.

The hoard contained coins of what all agree must be the first four || known issues of the Athenian New Style, coins of four of the eight known issues of Eretrian second-century silver, Chalcidian tetradrachms of the veiled head issue and an octobol of ΜΕΝΕΔΗ. All pieces were FDC. Miss Thompson argued that the Athenian coins were contemporary with the Euboean, that the Euboean coinages were initiated in 196 after the proclamation of Flamininus, that they only lasted a short time, and that therefore the Athenian coins were also initiated in 196.

Let us assume for a moment that the dating of the Euboean issues is sound. There would still be two escapes from the conclusion that the high chronology was sound in part or as a whole. It might be possible to argue that there were gaps in the Athenian sequence so large as to make the retention of both 196 and 87 possible; I do not find this very attractive. It could also be legitimately asked why the Anthedon hoard was entitled to special consideration. If at least ten other hoards are allowed by Miss Thompson to

[20] *Ibid.* 157; cf. *Chronologie Delphique* 63. [21] *Hesperia* Supp. 8 (1949), 6–30.
[22] First in *ANSMN* 5 (1952), 25–33, where the hoard is published.

have substantial chronological gaps, why not the Anthedon hoard? Would it not be better to close the gaps in the ten hoards and leave the single Anthedon hoard as a puzzle?

Such surgery may not be necessary if we re-examine the date of the Euboean issues on which Miss Thompson's whole structure depends. To take Eretria first, I cannot see that the almost universal agreement that its silver coinage was instituted in 196 is supported by more than an attractive historical attribution with no solidity behind it at all. As far as the internal chronology is concerned, it is clear from Wallace's discussion[23] that the silver could start in 196 with the Demarchos–Mantidoros bronze (and that date is only a guess) or with the Aristonikos bronze, dated by Wallace *c.* 175, as Newell[24] thought on the grounds of the similarity of the bull on the Aristonikos bronze to the bull on the silver octobols. Wallace rejected Newell's view, as far as I can see, because it seemed obvious to him that the silver coinage began in 196, the most natural moment, confirmed by Miss Thompson's conclusions about the Anthedon hoard.

These arguments are circular, and moreover dubious, since there happens to exist objective evidence of the date of the Eretrian silver, noted in fact by Wallace himself.[25] Three tetradrachms of this series were found in the Babylon hoard,[26] mentioned above in Section II as || containing an Attic tetradrachm dated by Miss Thompson 189/8. The hoard was certainly buried *c.* 150, and yet the Eretrian tetradrachms, as can be seen from Regling's plate,[27] are very sharp and have been damaged by burial, not by circulation. They had certainly not been in circulation for anything like forty-five years. Two of the magistrates represented at Babylon are also represented at Anthedon. This seems to me to throw some doubt on the proposition that the Anthedon hoard must have been buried *c.* 192 and that the Eretrian issues begin in 196.

A second point is of less importance. Robert[28] has shown that in 167 the treasury of Delos possessed a large sum in τέτραχμα καινὰ ταυροφόρα. He makes a strong case for saying that these are our Eretrian tetradrachms. Miss Thompson's view of the Eretrian coinage would appear to imply

[23] *The Euboean League and its Coinage* 124–6. [24] *NNM* 68 (1935), 17–19.

[25] *Phoenix* 4 (1950), 21 n. 1.

[26] Noe 116 = Regling, *ZfN* 38 (1928), 92–131. The Eretrian coins are described on pp. 113–14.

[27] Plate xi, nos. 56–8. [28] *Études* 146, 156–9.

coinage from 196 to 188, so that the coins would not be very new in 167. The language of inventories is not always reliable, but perhaps this evidence suggests a late date for Eretrian silver coinage.

The case for dating the Chalcidian issues is no stronger. The case for 196 is as weak or as strong. Gardner[29] suggested 192/1 and Antiochus' wedding as an occasion for them, a case which was only plausible as long as the veiled Hera appeared on only one issue. Newell again preferred to bring them down later.[30]

I have no wish to argue here for any particular date for the Euboean issues of the Anthedon hoard. But it does seem clear to me that to date them in 196 has no more than a superficial plausibility, that their date is not so firm as to justify their use for dating other coins, and that they should not be used as a peg to date the Athenian New Style.

VI THE AESILLAS OVERSTRIKE

If this is so, a coin used by Miss Thompson only as a secondary support for her chronology becomes of primary importance. This is the Berlin tetradrachm of ΔΗΜΕΑΣ–ΚΑΛΛΙΚΡΑΤΙΔΗΣ overstruck by Aesillas, quaestor of Macedonia. Aesillas' dates are given as 94–88 by all, but ΔΗΜΕΑΣ–ΚΑΛΛΙΚΡΑΤΙΔΗΣ belongs well down Miss Thompson's sequence. She dates the issue 107/6. I am satisfied that it would not be possible to move it more than four years back in her stylistic sequence without doing the utmost violence to her || arguments. On any calculation it must be ten years later than the coins which I have argued here to be connected with Sulla's sack of Athens. If it was overstruck between 94 and 88, the low chronology is in ruins, for all its attractions.

I note, and discard, two possibilities. There might be something wrong with the coin itself, though Regling passed it. The coin might be genuine, but a non-Athenian issue of uncertain date, like the issue of ΚΟΙΝΤΟΣ–ΧΑΡΜΟΣΤΡΑ excluded by Miss Thompson from the Athenian issues.[31] Neither view has great attractions.

There is a third possibility. If the arguments presented here for the low

[29] *NC* n.s. 18 (1878), 96–100. But add *ANSMN* 5 (1952), 26, no. 1, which does not seem contemporary.

[30] *NNM* 68 (1935), 18, 22.

[31] Pp. 464–7. I do not feel confident that this exclusion should be accepted.

chronology are sound, the Athenian coin is post-Sullan; therefore Aesillas is not a pre-Sullan quaestor of Macedonia, despite the virtually unanimous verdict that he was, but post-Sullan and probably later than 73.

Is this an impossible position to maintain? I do not think it is. Aesillas is himself otherwise unknown. He has to be dated by the whole group of Roman silver of Macedonia to which he belongs. Three types have to be considered.[32] The first has CAE. PR. MAKEΔONΩN on the obverse, AESILLAS Q. on the reverse. The second, the type of the overstrike, differs only in the omission of the obverse legend. The third, die-linked to the second, has no obverse legend and the reverse legend SVVRA LEG. PRO Q. The case for the orthodox dating is straightforward. Q. Bruttius Sura is known to have been legate in Macedonia 93–87.[33] The chances are obviously very high that he is the *legatus pro quaestore* of the coins. A L. Iulius Caesar was proconsul of Macedonia at some time.[34] If he is the CAE. of the coins, and shortly antedates Sura, he could be the consul of 90, for whom a praetorship in 95 and a proconsulship of Macedonia in 94 would be entirely reasonable. The identification of Sura gives a clear and plausible picture, which no one has doubted or would have doubted, had it not been for the difficulties about Athenian New Style chronology.

However, this picture does depend entirely on the identification of Sura. There is no reason to suppose that the L. Iulius Caesar who was proconsul of Macedonia must have been the consul of 90. || L. Iulius Caesar, consul 64, is, on our evidence, just as much in need of a praetorian command. He would have been praetor in 67 at the latest,[35] perhaps a year or so earlier, and in these years a yawning gap in the *fasti* of Macedonia will accommodate him without difficulty.[36] If Aesillas was his quaestor, the overstrike presents no difficulty to the low chronology.

There is, after all, another Sura, P. Cornelius Lentulus Sura, consul 71, the Catilinarian conspirator.[37] No direct evidence links him with Macedonia, but nothing is known of his activities between 70, when he was expelled

[32] Gaebler, *ZfN* 23 (1902), 171–9, *Die antiken Münzen von Makedonia und Paionia* i. 69–74; Head, *Historia Numorum*[2] 241.

[33] Broughton, *The Magistrates of the Roman Republic* ii, 15–16, 50.

[34] *IG* xii. 8 241, cf. 232; note the doubt raised by Broughton, 14 n. 3, about the praenomen and his correction of Gaebler's dates.

[35] Broughton, 143, with 150 n. 2. [36] Cf. Syme, *CP* 50 (1955), 130.

[37] *RE* iv, 1399–1402.

from the Senate, and 63, when he became praetor for the second time and was temporarily rehabilitated. He ought to have been doing something in these years to re-establish his reputation, and there is, as far as I know, no legal objection to the hypothesis that at some time in these years he held the position of *legatus* in Macedonia. Nor does one have to look very far to find a patron who would give him such a position in the time of his disgrace. At some time after the death of her first husband, M. Antonius Creticus, in 72–71, he married the sister of L. Iulius Caesar, who is the very man we are suggesting as the CAE. of the coins. It does not seem improbable that his brother-in-law would at this stage have been prepared to help him, to take him to Macedonia, where he can have been minting *pro quaestore* in direct succession to Aesillas. Caesar no doubt regretted the relationship later, but this is no reason against his having given him help earlier.

This is a consistent picture of the Macedonian issues. It is more hypothetical than the conventional picture, but it seems to me a necessary hypothesis. At some stage in our inquiry we have to accept some identification as solid. Which, after all, is more probable? That the issues of ΒΑΣΙΛΕ ΜΙΘΡΑΔΑΤΗΣ–ΑΡΙΣΤΙΩΝ are to be placed *c.* 121 against their conventional date of 87, or that the SVVRA of the Macedonian coins is not Q. Bruttius Sura? The first hypothesis, I have tried to show, produces almost universal confusion, the second, clarity and order.

There is, moreover, now, a tiny numismatic flaw in the conventional dating of the Aesillas issues. It lies in the hoard from Platania recently published by Mme Varoucha.[38] The hoard seems to have been buried *c.* 51. Coins of Gaius Julius Caesar of 54–51 were FDC. A denarius of Titius (*c.* 88, Sydenham) was, as one might expect, || mediocre in condition. The conventional chronology would have suggested that the two Aesillas coins would be roughly similar. They are, on the contrary, classed as 'bien'. If there were no other reason to suspect the chronology, this would pass without comment. As it is, I hope it adds some substance to my doubts.

CONCLUSION

In my view, then, Miss Thompson's dating of the New Style coinage from 196 to 87 has very little positive attraction, and creates a multiplicity of

[38] *BCH* 84 (1960), 496.

difficulties of all types. By comparison, a lower chronology, founded on the conventional date for Mithradates–Aristion, accommodates virtually all the evidence without difficulty.

My alternative chronology suggests a start to the coinage *c.* 164. The coinage was thereafter of considerable weight until the Sullan sack. It then continues on a very much reduced scale and peters out *c.* 50 or a little after.

In historical terms this means discarding the view that it was simply enthusiasm for Flamininus that led to the creation of the New Style. It now seems to me inevitable that its creation was caused by harder thinking than that. The establishment of Delos as a free port under Athenian control and the wrecking of Rhodes as a trading-power are the keys to the situation. Athens had once more a chance to be what she had been as a great trading-state. One essential element in her former strength had been the demand for her silver in the form of coin, and, *mutatis mutandis*, the policy, inferred by Kraay to have lain behind the issue of the first owls,[39] is in operation here. Athens was entering the battle once more, and, as an essential weapon, she brought her coinage up to date.

I hope I have here exhausted the material which is of direct value for the overall dating of the series. Some of the next moves, if my case is accepted, are clear. Surveys of the end of the Old Style and of the bronze seem to me urgent necessities. The Euboean issues and Aesillas will probably repay further examination. As far as the internal chronology is concerned, we now need a re-examination of the evidence of the coins for the calendar and of the calendar for the coins, this time on firmer foundations.[40] I have not attempted it here, because I thought it best for this purpose to treat Miss Thompson's internal || sequence as untouchable. I do not think that it should be so treated, and I would judge that there are some pretty problems coming in balancing the stylistic, numismatic arguments for sequences of ordinary and intercalary years against the epigraphic arguments. First, however, we must arrive at some measure of agreement about the dating of the series as a whole, and it is to this task that this long, but, I hope, not unprofitable argument has been directed.

[39] *NC* 6th ser. 16 (1956), 62–4.

[40] Meritt, *The Athenian Year* 180–91, working from Miss Thompson's dates, must be written off as an almost total loss, and I hope that it will not be regarded as sound method to argue from his partial success in accommodating these dates to his scheme to the validity of the dates.

I cannot close without making clear the enormous debt which I and all other students of this problem must feel to Miss Thompson. Without her work we should still be faltering in complete darkness. As it is, I hope it may not be entirely wishful thinking which sees light ahead at the end of the tunnel.

[NC] Editorial Note

Many, though not all, of the hoards cited by Mr Lewis are touched upon by Miss Thompson from her point of view in *NC* 7th ser. 2 (1962), 301–33. Readers may find it useful to have references to the pages where she does so.

Anthedon, 325; Tel Ahmar, 310; Salonika, 312; Kessab, 311; Delos Γ, and ΛΗ, 323; Zarova, 320 *passim* and 323; Halmyros, 323; Carystus I, 315; Piraeus, 315f.; Abruzzi, 312; Dipylon, 315; Anatolia, 314; Cretan II, 316; Delos Α, 315; Hierapytna, 313; Salamis, 314; Delos ΙΓ, 315; Beliza, 314; Greece? (Noe 462), 315; Babylon, 317; 1934/5, 315.

30

Review of *The New Style Silver Coinage of Athens* by Margaret Thompson

Of all Greek coinages, it is perhaps the Athenian New Style which offers the greatest opportunities and the greatest potential rewards. A hundred-odd issues, mostly, if not all, annual, are lavish in information which should assist their arrangement and dating; once firmly dated, they will fertilise many various fields, mint-organisation, the Athenian calendar, prosopography, economic history. The attempt to arrange them has been frequently made since Beule. His work and Head's have been most influential; Svoronos and Kambanis rendered great services, but never reached the end of their task; finally Bellinger in 1949 broke away from the conventional starting-date of 229 BC and laid down the lines on which future study should proceed.

It is to Bellinger that Miss Thompson's enormously useful work is dedicated. Conceived in the belief that a new corpus of the coinage was needed if any firm chronological order was ever to be established, it has been carried through with a devotion and care for detail beyond praise. 6,888 coins are listed and ordered, numerous false readings have been removed, for the first time the essential evidence has been made fully available. Completeness is obviously a mirage in the pursuit of any coinage, and I found an unlisted reverse in the second dealer's stock I looked at with the catalogue, but I think that we can be confident that everything of importance is there for the time being. Most of this review will regrettably be devoted to violent disagreement with the most important of the author's conclusions, but it is perhaps the warmest tribute that can be paid to her that her assembly of the evidence

* Published in *CR* n.s. 12 (1962), 290–2. (Reprinted by permission of Oxford University Press.)

[1] I owe these references to Mrs S. A. Adam.

is so complete and so clear that one who is not a numismatist can acquire enough grip of the material to venture to disagree.

It will be convenient to separate the internal arrangement of the issues from their absolute dating. The author divides the coinage into three periods. Her Early Period includes twenty-eight issues with the monograms or abbreviated names of two magistrates. These are assigned to twenty-eight consecutive years on the assumption that no issue is missing (perhaps a shade hazardous, since || one issue is known from one coin first published in 1949 and the same thing might happen again) and given a definite order. This order is established by arguments of varying value. Die-links firmly establish MIKI–ΘΕΟΦΡΑ as the last of these issues and give us one pair of issues; a recut die gives us another pair. Hoards help a little, but the main weight of the argument rests on an extraordinarily rich and ingenious discussion of the development of the style of the obverse dies. This gives high probabilities, but I do not see how it can provide certainty. Others might work in different ways and get different results. To take one point of wider significance, ΚΤΗΣΙ–ΕΥΜΑ and ΓΛΑΥ–ΕΧΕ are dated in consecutive years on purely stylistic grounds. Both these years are shown by the appearance of N among their month-letters to be intercalary. Some have denied that Athens ever had two consecutive intercalary years, and for their benefit it should perhaps be pointed out that there is no overlap of control-letters between these issues. If we arranged the early issues by the control-letters they use, as the author herself does in her Late Period, we should get different results (though we should run into immediate trouble with ΜΙΚΙ–ΘΕΟΦΡΑ and ΗΡΑ–ΑΡΙΣΤΟΦ, who have no overlap of control-letters, but are die-linked). It does not seem to me inherently less probable that a die-cutter returned to the mint after a year or so's absence than that the mint used entirely different sources of silver in consecutive years (to adopt Miss Thompson's highly plausible interpretation of the control-letters). To speak generally, it would have been better if we had been given somewhere a guide to the degrees of certainty with which the order of these early issues had been fixed.

With the Middle Period, thirty-seven three-magistrate issues, we are on much firmer ground. Twenty-five of these issues are involved in die-links, and the points of uncertainty can be clearly defined. But the uncertainties do not appear in the tabulated results, with the effect that the author can deceive herself. For example, contrast p. 612, where 154/3 appears as an

intercalary year of whose position she is quite confident, with what is said about the placing of ΚΑΡΑΙΧ–ΕΡΓΟΚΛΕ on p. 312.

Her discussion of the forty-five remaining issues is revolutionary, and on the whole convincingly so. She shows that, surprisingly, there is an overlap between the three-magistrate and late two-magistrate issues and that the crucial ΒΑΣΙΛΕ ΜΙΘΡΑΔΑΤΗΣ–ΑΡΙΣΤΙΩΝ issue belongs stylistically with the last of the three-magistrate issues rather than somewhere in the two-magistrate issues. The problems of the first fifteen issues of this Late Period are akin to those of the Middle Period. The issues are large and there are many die-links; there are uncertainties, but not many. The last thirty are totally different. They can be grouped, but not ordered with any certainty at all. They survive in tiny quantities, five of them in only one coin, so we may have lost some issues, even if Athens coined every year in this period. Miss Thompson does wonders with them, but it is difficult to think after reading p. 400 that they should really have been given the dignity of a fixed order and fixed dates. She does, however, make it reasonably certain that theories which require a large gap anywhere in the series are untenable.

We can now turn to the problem of absolute chronology. After showing that the ΚΟΙΝΤΟΣ–ΧΑΡΜΟΣΤΡΑ and Ο ΔΕΜΟΣ strikings are non-Athenian, she is left with 110 regular issues which she dates from 196/5 to 88/7. The initial date of 196/5 she argued for in an article of 1952, and in it she shows a nearly absolute faith. Her only qualification is an occasional recognition of the || possibility of 197 (e.g. p. 107), which would incidentally dissolve one or two problems in the calendar. What are the consequences? ΒΑΣΙΛΕ ΜΙΘΡΑΔΑΤΗΣ–ΑΡΙΣΤΙΩΝ, which has always been placed by the clear historical requirements in 87, moves to *c.* 121 to commemorate an unknown benefaction of Mithradates V. Even more serious, the hoard-evidence, which I cannot analyse fully here, seems to reproduce this 30–35 year gap. Every hoard which contains, besides Attic New Style, coins which are firmly datable, shows, on Miss Thompson's dating, a substantial time-lag between the latest Attic, often in excellent condition, and the burial date of the hoard. Not a single Attic coin found at Delos, except the ΜΗ–ΜΗ issues shown by her to be Sullan, is allowed to post-date 112/11.

For these phenomena the author offers various explanations of varying attraction, but never considers the one which has been forced on this reader, that her whole chronology is 30–35 years too high, that Attic New Style does not reflect enthusiasm for Flamininus, but a hard-headed attempt to reflect

in an up-to-date coinage the new opportunities opened by the recovery of Delos; that Athenian New Style in fact ran from *c.* 165 to *c.* 44. There are two points and two points only which stand in the way of this hypothesis. The first is the Anthedon Hoard, containing the earliest New Style together with Chalcidian and Eretrian issues generally dated *c.* 196. But there is no objective evidence for this date whatever; the only objective evidence I can find points the other way, for similar Eretrian tetradrachms in the Babylon Hoard (Noe 116) buried *c.* 150 are in excellent condition. The second is more serious. A coin of ΔΗΜΕΑΣ–ΚΑΛΛΙΚΡΑΤΙΔΗΣ, dated by Miss Thompson to 107/6 and not capable of being moved more than four years earlier in her sequence, was overstruck by Aesillas, quaestor of Macedonia, at a date always given as 92–88. The case for dating the Macedonian coins is purely prosopographic and circumstantial; I believe that an alternative dating is possible and should be found.

This basic difference of opinion must not be allowed to cloud the solid merits of Miss Thompson's work. Her discussion of the mint-magistrates is eminently sensible; the careful work on the distribution of coinage year by year and within the year and on the length of life of dies is of the greatest possible interest. But, if the considerations sketched above are valid, her historical, prosopographic, and calendar conclusions are totally vitiated by her absolute chronology; in these departments she has only laid the essential foundations for further progress which will certainly be made and could not have been made without her work.

German printers and American block-makers are to be congratulated on a remarkable achievement with very difficult material; my list of misprints is tiny.

31

The Persepolis Fortification Texts

With over 2,100 texts published, the Persepolis Fortification Texts in Elamite, transcribed, interpreted and edited by the late Richard Hallock, already form the largest coherent body of material on Persian administration available to us;[1] a comparable, but less legible, body of material remains unpublished, as does the smaller group of Aramaic texts from the same archive. Essentially, they deal with the movement and expenditure of food commodities in the region of Persepolis in the fifteen years down to 493.

There is no up-to-date general account of these texts. Hallock's own contribution to *Cambridge History of Iran* ii (1985) is unchanged from a preprinted version circulated in 1971, and a good deal has happened since then. Besides substantial linguistic contributions by Gershevitch and Hinz, I single out one article by Hinz on the details of the administration,[2] one by Dandamaev on dependent populations,[3] one by Sumner on the settlement-patterns of the Persepolis plain.[4] There is a book-length treatment of the evidence of the tablets for religion by Koch.[5] The names in the tablets have been fully discussed by Mayrhofer,[6] but we badly need a prosopography; most of my own published work on the tablets concerns prosopographical matters.[7] Some of the material has begun (very slowly) to enter the more

* Published in *Achaemenid History*, iv. *1986* (1990), 1–6 (Nederlands Instituut voor het Nabije Oosten). Some references which were given in the text there have been transferred to footnotes here, and edited to match the style of this volume.

[1] Tablets with the prefix PF are from Hallock, *Persepolis Fortification Tablets*; those with PFa are from Hallock, *CDAFI* 8 (1978), 109–36.

[2] W. Hinz, *ZAssyr* 61 (1971), 260–311.

[3] M. A. Dandamaev, *Altorientalische Forschungen* 2 (1972), 71–8.

[4] W. Summer, *AJA* 90 (1986), 3–31.

[5] Koch, *Die religiösen Verhältnisse des Dareioszeit*.

[6] Mayrhofer, *Onomastica Persepolitana*.

[7] *JHS* 100 (1980), 194–5 [= this volume, 342–4]; 'Postscript 1984' in Burn, *Persia and the Greeks* 587–609; *The Greek Historians . . . A. E. Raubitschek* 101–17 [= this volume, 345–61].

general literature.[8] It is not only the text of the tablets which is important. The sealings which they bear provide one of our largest coherent bodies of seal material, capable of throwing a flood of light on seal-usage and art-history. Hallock devoted a preliminary article to this;[9] a full publication is in the hands of Margaret Cool Root.[10]

In this paper, no full treatment is attempted. I propose rather to attempt the general theme of what may loosely be called the state-economy and the pre-occupations of royal officials with the production of food and other materials. Such a theme may give the best introduction to the bulk of what the tablets have to tell us.

Firstly, they make it absolutely clear that everyone in the state sphere of the Persian economy was on a fixed ration-scale, or rather, since some of the rations are on a scale impossible for an individual to consume, a fixed salary expressed in terms of commodities. The most prominent figure in the texts, Parnaka, loosely speaking, the chief economic official of Persis, apparently || the king's uncle, is entitled to and gets, no matter where he is, a daily ration of 2 sheep, 90 quarts of wine and 180 quarts of flour. Gobryas, Darius' principal helper, father of Mardonius, appears to have been on a still higher scale.[11] At first, it was thought that these large rations were to enable the high personage to feed his household, but this view seems partly contradicted by the tablet PF24: 'Daily by Parnaka together with his boys 480 quarts (of flour) are received. By Parnaka himself 180 quarts are received. By his 300 boys 1 quart each is received.'

These 'boys' take us to the other end of the social scale. 1 and 1½ quarts of flour are near the norm, but there is evidence for very marked differentiation by skill, age and sex.[12] To take a group of what appear to be garment-workers, with unintelligible specialities (PF999), three males are on 45 quarts a month, 17 women on 40, 2 men and 36 women on 30, 3 women on 25, 15 mixed on 20, 2 boys and 1 girl on 15, 6 boys and 5 girls on 10, 2 boys and 5

[8] Hinz, *Darius und die Perser*; Cook, *The Persian Empire*; Dandamaev, *Political History of the Achaemenid Empire*. By contrast, *Cambridge History of Iran* ii, and Frye, *History of Ancient Iran*, are almost unaffected by the decipherment of the tablets.

[9] In Gibson and Biggs (eds.), *Seals and Sealing in the Ancient Near East*.

[10] [See Garrison and Root, *Persepolis Seal Studies*.] [11] Lewis, *Sparta and Persia* 4–5.

[12] The material is unknown to L. Foxhall and H. A. Forbes, *Chiron* 12 (1982), 41–90, now the standard treatment of cereal consumption in classical antiquity, and is perhaps even better adapted than the classical material for scrutiny by their methods.

girls on 5, but the boys and girls have no trade designation and are probably very young. There are odd commoners who get up to 60 quarts. In this social scale sheep rations are very uncommon and, when they come, they can be of the order of 1/9, 1/15, or even 1/30 of a sheep a month. Wine and beer have not been studied thoroughly, and, when they come, they are to some extent extras. However, travelling is recognised as thirsty work; see the 1,500 *lin*-makers going from Persepolis to Susa drawing roughly a third of a litre each per day (PF1542).[13]

The payment of rations is very highly organised. Travellers along the road carried sealed documents issued by the king or officials of satrapal level stating the scale on which they were entitled to be fed.[14] Tablets sealed by supplier and recipient went back to Persepolis as a record of the transaction. But we also have extensive information about work-groups, designated rather mysteriously by the names of the officials who 'assign' or 'apportion' them. Their ration-statements enable us to build up a remarkable picture of the size of operations involved. Dandamaev has produced tables to show that the tablets have 15,376 people on the pay-roll, attached to 108 villages, over the period 509–494, and has found 8,728 in the 23rd year alone.[15] With some qualifications, the general scale must be right, and so are his general observations about the balance between men and women and the number of children on the pay-roll. These are, broadly speaking, dependent populations, || not paid workers, whatever terminology of dependence we think appropriate. *Garda/kurtash* need not reflect the same kind of dependency everywhere, and we have no reason to think that these people had been hijacked and branded like those of Arsames in Egypt.[16] Equally, we have no reason to think that they were not, and some of the very large groups with foreign ethnics are clearly at least transplanted populations.[17]

Sometimes the information about these work-people is sufficient, with qualifications about our understanding of the vocabulary, to build up quite a detailed picture about the range of activities going on in a particular place or

[13] In the later Treasury Texts deficient rations can be made up in silver (Hallock, *JNES* 19 (1960), 90–100); this is harder to demonstrate for the period of our texts, but see *Persepolis Fortification Tablets* 32–3, for partial payments, and PF1946. 4–12.

[14] It is these documents which provide us with new evidence for satraps, satrapies and (a topic hardly touched as yet) the king's movements.

[15] *Altorientalische Forschungen* 2 (1972), 71–8.

[16] Driver, *Aramaic Documents* VII.

[17] Lewis, *Sparta and Persia* 6–7.

area.[18] At Urunduš in 495 (PF864), there were 77 workers disposed as follows: 2 armour-tailors, 20 treasury-guards, 7 craftsmen, 19 furniture-makers, 14 wood-porters, 1 house-servant, all male, together with 7 boys, 4 women and 3 girls. At Shiraz (PF865) there were 181 workers: 1 armament-protector, 6 camp-administrators, 1 metal-forger, 2 artists, 10 treasury-guards, 4 craftsmen, 5 furniture-makers and 5 house-servants. But here the women are active as well, with 1 chief, 51 artists and 45 craftsmen, with equal pay to men of the same designation, plus 2 wetnurses, 11 cooks and 8 girls of various ages. Rakema (PF866) was even larger, with 311 workers. These establishments can almost be described as factories.

In relation to the work of these factories, we have texts about skins, because animals also fall within the scope of the archive, under the general supervision of a 'cattle-chief'. Here again, we get some very high figures. See the account texts, PF2007ff., mostly only for sheep and goats.[19] PF2007 lists 16,843, by goats and sheep, with 2 male and 3 female categories for each. PF2014 is a small stockyard for bigger animals, running down gradually from 35 to 12. Bird farms are also visible. All these turn up both with accounts of actual animals, and with the animals as recipients of grain-rations. The meticulous nature of the accounting aimed at is most clearly seen in the bird-texts, above all in PF1943.21–30, which lists ten different kinds of birds, drawing rations from 1 quart down to 1/50 quart a day. Hallock thinks the biggest rations go to duck and geese; I am still looking for an ostrich.

These are animals which are important to the ration-administration in terms both of input and output. Others appear only in terms of input, and we have quite frequent references to rations for horses, more infrequent for camels. Royal horse-rearing is known from the Greek literature,[20] and other areas, notably Media, had better grazing and larger establishments than anything possible in Persis. But PF1793 gives us an establishment where 135 men are looking after the horses and mules of the king and the princes, though we cannot see more than 90 horses in any one place (PF1769) and most of the detail comes from places where what we are || dealing with is recognisably a detachment of post-horses. One of the more engaging features of our texts is the frequency (PF1757ff.) with which some, evidently rather special horses, get wine-rations (cf. Hom. *Il.* VIII.189).

[18] Hinz, *ZAssyr* 61 (1971), 266ff. [19] P. Briant, *JESHO* 22 (1979), 136–61.
[20] *Ibid.* 159–60.

Post-horses will be in stations along the royal roads, and the supplying and auditing of such stations is a major preoccupation of the ration-administration. Some of these have very little to do, and there is some reason to suspect that the administration had provided them over-generously. PF1969, 1970, 2077 form a recognisable group of stations, set up with an original float in Darius' 14th year and given a regular allocation of grain for three years. After three years in which very little call seems to have been made on their services, they are all closed down and their balances transferred elsewhere.

Others are a great deal livelier, particularly those which include industrial establishments as well or which also serve as supply-points for work parties in their area. The journal documents tend to be quite long, and recognisably summarise the sort of transactions which we find on individual tablets. A big station of this type hardly ever gives us a complete or balanced record of corn or flour, because the account will have been on several tablets, arranged to some extent according to the authorisation for the rations; consequently, our texts may show regular payments or travel payments, but not both. The wine tablets (e.g. PF430) give us a simpler and clearer picture of the mixture.

It is now clear that all journal-texts (v texts) originally included an account-text (w texts), sometimes on the same rectangular tablet, sometimes separately. It is these account-texts which ought to provide the evidence as to where the commodities are coming from to keep these places going. But essentially they do not. They do go into detail when recording a transfer from another similar place, but these receipts are always relatively small compared to the items which are simply labelled 'provisions' (*haduš*). 'The ultimate source of the *haduš* receipts is never indicated. Presumably the source was the same superior agency as that to which the account was sent, and hence there was no need to specify it.'[21]

This is a maddening situation, but I find it hard to believe that the supplies are coming to any large extent out of the local private sector, whether by purchase or by tax. Persis should in theory not be paying tax in any case (Her. III. 97.1). This is unlikely to be totally true,[22] and at least the fourth-century texts seem to attest that sheep and goats were occasionally received as tax, but there are no corresponding texts about corn and wine that we can see. It is clearly possible that some comes in from elsewhere, above all from

[21] Hallock, *Persepolis Fortification Tablets* 59. [22] H. Koch, *ZAssyr* 70 (1980), 105–37.

Babylonia. But that there are royal estates producing foodstuffs within the system is clear enough, above all from the quite extensive F texts where considerable quantities are set aside for royal officials for seed. Normally, 10 per cent is set aside thus, but for certain grain this sinks to 3.33 per cent.

I can only assume that the mass transactions which transfer commodities || from the place where they are grown or from outside the area formed a different part of the documentation. It is after all clear that clay tablets show only part of what is going on. Ample evidence exists that even at Persepolis there are 'Babylonian scribes writing on parchment', presumably in Aramaic.[23] Perhaps these recorded the larger amounts.

Very little in these texts concerns the army, but it is hard to believe that it was unbureaucratic. In fact, we know that it was not, since we have Aramaic papyri from Elephantine showing very similar scales and behaviour for the garrison there.[24] It seems entirely probable that the large ration dumps built up in Europe for the expedition of 480 (Her. VII.25.2) involved an extremely large amount of paper work, both in their supplying and in their disbursements.

We should not assume that paper work was always immaculate. One not infrequently finds cases where errors in sub-totals have passed unnoticed and been carried over into grand totals and cases where accounts had not been made up for some years. Occasionally, even the administration becomes aware of serious difficulties:

> Tell Mirinzana, Šakšabanuš spoke as follows: To thy *tartammatti*. A sealed document concerning (the fact) that the accountants are not delivering the accounts was sent to Parnaka. The man who carried (?) that tablet, that delivery-man (?) fled away. Now do you seize that man and send him forth (to) Media. (In) Media (there will be) a close questioning of him (lit. his oil [?] it squeezing). Furthermore, when you send forth a tablet from you to Parnaka, then write on the tablet the name of the man who is carrying the tablet, (and) send (it) forth. Thus Parnaka ordered and I made it. Formerly the name (of) that man was not written. (PFa28)

It should be clear that the contribution of the tablets to historical knowledge is still at a very early stage. I have already indicated some points where

[23] Two Akkadian tablets are so far known from Persepolis, one public, one private (M. Stolper, *JNES* 43 (1984), 299–310).

[24] Cowley, *Aramaic Papyri* 24; Porten, *Archives from Elephantine* 80ff.

work is already in progress or remains to be done. It cannot yet be foretold what contribution may be made by the unpublished tablets, not necessarily individually, but because the addition of new documents to our existing structure must have an enriching effect on our picture of the whole. Apart from a few places in Babylonia for short periods, Persepolis is now the best-documented area in the Achaemenid empire. What generalisations or other insights this provides for other areas is perhaps likely to remain one of the main methodological problems for Achaemenid scholarship.

32

The King's dinner (Polyaenus, IV.3.32*)*

This seminar has rightly laid stress on the independence of oriental evidence and on the importance of freeing its interpretation from presuppositions based on Greek evidence. I must admit that much of my recent thinking has also gone in the other direction. Taking my start from what seemed to me to be the fact that Greeks were widely employed by Persians in a secretarial capacity,[1] I have argued in general terms that Herodotus could have had good evidence for his more documentary material and, more specifically, that there is reason to believe that Xerxes' army-list contains sound prosopographical information.[2] Despite the sweeping condemnation which fourth-century Greek writing about Persia has sometimes received,[3] I think that there is some evidence to suggest that factual investigation of Persian institutions continued. I propose to discuss here the most substantial piece of evidence. Surprisingly, since it is preserved in medieval manuscripts, it has had virtually no discussion at all;[4] if it had been a new discovery on papyrus, it would have had a lengthy bibliography.

Polyaenus, IV.3.32, gives us a list of commodities prepared for the King's dinner and supper read by Alexander in the Persian palace, written on a bronze pillar, where there are the other laws which Cyrus wrote. Three Teubner pages of straight document are then followed by a short anecdote,

* Published in *Achaemenid History*, ii. *1984* (1987), 79–87 (Nederlands Instituut voor het Nabije Oosten). Some references which were given in the text there have been transferred to footnotes here, and edited to match the style of this volume. Tablets with the prefix PF are from Hallock, *Persepolis Fortification Tablets*; those with PFa are from Hallock, *CDAFI* 8 (1978), 109–36.

[1] Lewis, *Sparta and Persia* 12–15.

[2] Lewis, 'Postscript 1984' in Burn, *Persia and the Greeks* 597–602; and in *The Greek Historians . . . A. E. Raubitschek* 101–17 [= this volume, 345–61].

[3] Momigliano, *Alien Wisdom* 132–5.

[4] An anonymous contribution to the *Classical Journal* 30 (1827), 370–4, condemned it as inauthentic. Thereafter it barely appears in the literature until it served Peter Green for a picturesque footnote (*Alexander of Macedon* 303).

recounting how the other Macedonians regarded the list as a sign of the King's *eudaimonia* and Alexander treated it with contempt. The sources of Polyaenus are not always straightforward.[5] As one might expect, the Alexander anecdotes show some affinity with the vulgate tradition about him, and an ultimate origin for them in Cleitarchus is at least a possibility.

Two new discoveries make it desirable to reconsider this text. The first is the text describing how Ashurnaṣirpal II dedicated his palace at Nimrud with a ten-day feast for nearly 70,000 people.[6] Even without sophisticated lexicographical work, at least seventeen items on the menu overlap. Polyaenus is describing a quite plausible Near Eastern feast. Closer in date and relevance are the Persepolis Fortification || Tablets. These also exhibit a range of foodstuffs which show a considerable affinity with those recorded in the document. It will be more convenient to relegate the detail to an appendix, and others are more capable than I am of doing the detailed work. I had at any rate convinced Hallock before he died that the text contains some very helpful and suggestive matter which offers the hope of solving some of the more intractable problems in the Elamite lexicography of foodstuffs. The further point, of course, is that the Persepolis texts make it clear that Achaemenid administration is likely to have contained such records laying down fixed ration scales for persons on every level; there is no reason to think that the King was excluded. To my mind the really suspicious feature is the bronze pillar, although some of the quantities are pretty frightening. There will be some temptation to wonder whether the record of a special occasion has been taken as normal.

We can have no certainty that the text derives from Cleitarchus and many of us doubt whether Cleitarchus can plausibly be credited with first-hand research in oriental sources. It seems more likely, as Lane Fox suggested to me, that whichever historian brought the documentary core into connection with Alexander had taken it over from a Greek source in which it had already appeared. Our knowledge of such sources, with further evidence that the King's household economy was discussed more seriously than appears in Polyaenus, virtually all comes from the *Zitatennest* at Athenaeus, IV.145–6. After various unspecific quotations on Persian luxury, we get a very

[5] Nothing serious has been attempted since Melber, *Jahns Jahrbuch* Supp. 14 (1885), 415–688.

[6] D. J. Wiseman, *Iraq* 14 (1952), 24–44; Pritchard, *Ancient Near Eastern Texts*[3] 560; Grayson, *Assyrian Royal Inscriptions* ii, §682.

long piece from the fourth-century Heraclides of Cumae (*FGrH* 689F2) with a full account of the King's dining procedure and some serious discussion. The King's dinner, he says, may sound imposing, but, if examined closely, it will turn out to be organised economically and accurately both for him and for other powerful Persians. He gives us an account of what is prepared for the King's dinner, more sketchy than the one in Polyaenus, and then says that much of this in effect goes on salaries for the diners and for the King's bodyguard. Mercenaries in Greece get paid in cash, but in Persia they get food as pay. The observation about food as pay is certainly sound, but it is by no means clear that Heraclides is quite right about large food allocations. In the initial stages of work on the Fortification Tablets Hallock had considered it possible that the very large rations given to Parnaka, the King's uncle, were for him to feed his staff, but he abandoned this later when a new tablet showed Parnaka's 300 boys getting normal meal rations alongside Parnaka's inflated scale.[7] It might be possible to save Heraclides' credit in part by supposing that the distributions were in fact only of meats, very rare at Persepolis for the rations of ordinary people, but he certainly contemplates the distribution of loaves after dinner as well, and there is surely some misunderstanding. Nevertheless, the passage is still quite impressive and is doubtless responsible for Meyer's belief, so different from that of Momigliano, that || Heraclides and Deinon operated 'in durchaus wissenschaftlicher Weise'.[8]

There was yet another approach possible to the size of the King's dinners. After some Herodotean material on the King's dinner and its costs, Athenaeus quotes Ephippus (*FGrH* 126F2) on the cost of Alexander's dinners and makes a guess at the number of consumers, followed by Ctesias (*FGrH* 688F39) and Deinon (*FGrH* 690F24), evidently in agreement, on the numbers at Persian royal dinners and their cost. This figure is then reduced to *Italikon nomisma*, and it is thereby demonstrated that Alexander's dinners cost just as much per diner as the King's had done. I can offer no suggestion at all as to who may be Athenaeus' source for this calculation and conclusion.

In our Polyaenus text, there is no attempt to reduce these ration amounts to any cash equivalent, no explicit indication of how many people the rations described would feed. There are traces of some form of editing. Some notes

[7] Hallock, *Persepolis Fortification Tablets* 23 with n.; PFa 4.

[8] *Geschichte des Altertums* v[4].332.

may go back to the original document, for example, the totals and various notes that some commodities vary slightly according to where the King is. But there are also linguistic glosses for a Greek audience, the giving of equivalents for Persian measures in Attic terms, and one possible additional explanation of use; all these belong to the process of transmission.

It seems certain to me that we are dealing with a fourth-century Greek source. As far as we are concerned, there are three main possibilities: Ctesias, Heraclides and Deinon.[9] I am inclined to rule out Heraclides, since we have one general account of the King's dinner from him already, and neither the numbers for daily consumption of animals nor the animals themselves quite correspond. Deinon is more difficult. Of the three fragments in which he deals with the King's dinner, F24, as we have seen, is only about numbers and cost and has no bearing on our text, F12 begins with a quite different point about the variety of origins of the foodstuffs, a matter on which our text is virtually silent, and F4, though visibly from the same world as our text, seems to have a different context. On the whole, the case is the strongest for Ctesias. In his work *About the Tributes in Asia* (*FGrH* 688F53) he did list 'everything which is prepared for the King at his dinner'. All we know about that list is that it included neither pepper nor vinegar. Pepper is indeed absent from Polyaenus. It must be admitted that vinegar is there (D10), but there is another escape-route besides the easy one of saying that Athenaeus or his source is being careless. It does not appear in the part of the list which the King consumes himself, but only among the commodities which he distributes.

No claim of Ctesias to have worked on administrative documents survives, and I am more than sceptical about his assertion (*FGrH* 688F5) that he had || studied the royal parchments on which the Persians had collected ancient actions, according to some law. The pedigree of our document is hardly clear, but it does not seem to be a total invention. My conclusion remains that fourth-century Greek historiography continued to use at least some Persian documentation and that Heraclides, whether using documents or not, was at least capable of talking sensibly about Persian institutions.

[9] Pierre Briant suggested that I had been too hasty in ruling out Chares of Mytilene, whose position as Alexander's major domo might have given him access to such a document. All I could say in reply was that he did not seem a very likely source for Polyaenus.

APPENDIX

The text is by no means easy to organise, though certain groupings are clear. Ashurnaṣirpal's order is: meat, poultry and fish, bread, drink, condiments, vegetables and fruit.

a = artaba(i). m = maries. t = talent. mn = mnai. kap = kapeties.

A	1	Wheat-meal, pure	400 a
	2	Wheat-meal, second class	300 a
	3	Wheat-meal, third-class	300 a
		Wheat-meal, total for dinner	1,000 a
	4	Barley-meal, very pure	200 a
	5	Barley-meal, second class	400 a
	[6	Barley-meal, third class	400 a]
		Barley meal, total	1,000 a
	[7?	*Semidalis*	?]
	8	Groats made from *olyra*	200 a
	9	Fine flour from *alphita*, for possets	10 a
	10	Chopped cardamum, sifted fine	lost
	11	*Ptisane* (treated barley?)	10 a
	12	Mustard-seed	⅓ a
B	1	*Probata*, male	400
	2	Cattle	100
	3	Horses	30
	4	Fatted geese	400
	5	Turtle-doves	300
	6	Various small birds	600
	7	Lambs	300
	8	Baby geese	100
	9	Gazelles	30 ||

c	1	Today's milk	10 m
	2	Sweetened sour milk	10 m
	3	Garlic	1 t
	4	Onions, pungent	½ a
	5	*Phyllon* (silphium-fruit?)	1 a
	6	Silphium-juice	2 mn
	7	Cummin	1 a
	8	Silphium	1 t
	9	Oil of sweet apples	¼ a
	10	Posset from sour pomegranates	1 a
	11	Oil of cummin	¼ a
	12	Black raisins	3 t
	13	Flower of anise	3 mn
	14	Black cummin	⅓ a
	15	Seed of *diarinon*	2 kap
	16	Sesame, pure	10 a
	17	*Gleukos* from wine	5 m
	18	Cooked round radishes in brine	5 m
	19	Capers in brine, from which they make sour sauce	5 m
	20	Salt	10 a
	21	Ethiopic cummin	6 kap
	22	Dry anise	30 mn
	23	Celery-seed	4 kap
	24	Sesame oil	10 m
	25	Oil 'from milk'	5 m
	26	Terebinth oil	5 m
	27	Acanthus oil	5 m
	28	Oil from sweet almonds	3 m
	29	Dried sweet almonds	3 a
	30	Wine	500 m

(When he is in Babylon or Susa, he has half his wine from palms, half from vines.)

	31	Wood (*xyla*)	200 waggons
	32	Wood (*hule*)	100 waggons
	33	'Raining honey'	100 square cakes

(When he is in Media, he distributes *tauta*.) weighing 10 mn each

	34	Safflower seed	3 a
	35	Saffron	2 mn

(All this for drink and *ariston*)

He distributes:

D	1	Wheat-meal, pure	500 a
	2	Barley-meal, pure	1,000 a ||
	3	Barley-meal, second class	1,000 a
	4	*Semidalis*	500 a
	5	Groats made from *olyra*	500 m
	6	Barley for the animals	20,000 a
	7	Chaff	10,000 waggons
	8	Straw	5,000 waggons
	9	Sesame oil	200 m
	10	Vinegar	100 m
	11	Chopped cardamum, fine	30 a

All this he distributes to the soldiers (?)
This is what the King consumes in a day, including his *ariston*, his *deipnon* and what he distributes.

GENERAL It would be hard to imagine a text more vulnerable to omissions and transpositions. A6, absent in the MSS, was an obvious omission picked up by the earliest editors and it would be reasonable to assume that there may be more omissions which are less obtrusive. I have ventured to add A7, to improve parallelism with D4; I see no reason to deny the King this delicacy.[10] There are few obvious inconsistencies, but it seems unlikely that A8 should be in dry, D5 in liquid measure. In view of the inconsistency between the beginning, the note after Section C and the note after Section D, it may be that we have lost an explicit distinction between *deipnon* and *ariston* after Section B. The note after C33 is ambiguous.

CEREALS (A1–12, D1–6) There is no doubt that the predominant cereal at Persepolis is barley. Hallock's translations conceal this fact[11] because of the occasional careless use of ŠE.BAR for totals which include other cereals. In this text it only appears unprocessed at D6. 20,000 artabai is a very large figure, a day's ration for 200,000 ordinary horses or 300,000 mules. Before we condemn the figure or the document out of hand, we have to consider what other baggage animals there were and what animals for food there might have been in the establishment which it was inconvenient to graze.

Human beings get meal (ZÍD.DA), not normally specified as to type or quality in Persepolis receipts. The most tempting parallel for our text might come from the Royal Provisions texts PF699–700, and Hinz has not hesitated to translate the three adjectives for meal given there as 'good', 'better', 'best'.[12] I retain a doubt, since the

[10] Cf. Lewis, *Sparta and Persia* 60. [11] See *PFT*, 76.
[12] *Neue Wege im Altpersischen* 81.

categories are not counted separately. Other texts do distinguish quantitatively between *bašur* meal and *ramiyam* meal (both in PF326), and in Driver, *Aramaic Documents*, VI Neḫtiḫur is to get two measures of *ḥwry* (white) meal, which seems to correspond to our text's 'pure' (*kathara*) and three of *rmy* meal. Hinz has discussed the uses of *ramiyam* etc. || and concluded that it means 'fein'.[13] But there seems to be an equal temptation to think that *bašur* is something special, which may be connected with its different uses in PF302, 1854. My brief and inexpert search in Neo-Babylonian texts has found no parallel for grading meal with ordinal numbers: what comes to my mind is PT85, with its distinction between white, second-class and third-class silver.

There are ample Elamite words to supply our range of cereals, with over twenty awaiting definition. I can only refer to Hallock, *PFT*, and Hinz, *Neue Wege*, for discussion, allowing myself merely to wonder whether ŠE.SA.A is anything to do with our AII, since it certainly seems to arise from processing barley (PF430).

At Persepolis, even Parnaka only gets 18 BAR = 6 artabai of meal a day; I have guessed that Gobryas got 20 BAR.[14] The King is certainly operating on a different order of magnitude altogether. As usual, it is tempting to think of his household as distinct from his soldiers, but this may be special pleading. As far as the soldiers are concerned, we are on firmer ground. DI–4 add up to 3,000 a = 9,000 BAR = 90,000 QA. If we assume an average ration of 1½ QA a day, that implies 60,000 soldiers. Again, this seems high, but we may be underestimating the ration for elite troops.

ANIMALS (BI–9) This is the passage closest to Heraclides, but different from him. He says that a thousand victims a day are butchered for the King. These include horses, camels, oxen, donkeys, deer and *ta pleista probata*. Many birds are consumed too, Arabian ostriches, geese and chickens. The birds are evidently not included in the thousand, so the comparable figure here, if there are no omissions, as there may be, is 860. The best parallel for the number is not the two sheep a day received by Parnaka as a regular ration, but the 100 sheep received by the lady Artystone, together with 200 m of wine, apparently for a special feast.[15] If we divide Ashurnaṣirpal's figures by ten for the ten days of his feast, the figures are fairly comparable, except that the exceptional nature of that occasion demanded a much larger provision of sheep and lambs.

I have left *probata* untranslated and assume that it contains both sheep and goats, a common Greek usage, paralleled by the similar use at Persepolis of UDU.NITÁ. No horses appear as foodstuffs at Persepolis, and it would be unsafe to

[13] *Ibid.* 40. [14] In Burn, *Persia and the Greeks* 595.

[15] G. G. Cameron, *JNES* 1 (1942), 214–18; *PFT*, 52 n. 48; W. Hinz, *ZAssyr* 61 (1971), 288.

assume that those which are fed there are used for anything except transport. Birds as royal provisions appear in PF697–8, and there is ample evidence for their feeding (PF1718–56). *datmakaš*, interpreted by Gershevitch as 'puffed up',[16] seems a reasonable translation of *siteutoi* (B11). (Consider also the uses of IN.lg and *kibatna*.) 'Various small birds' (B9) translate readily into *kuktukka* fowls (*PFT*, 49). Ashurnaṣirpal's feast involved || 500 gazelles; I do not see any at Persepolis, though *šukur* is a possibility (cf. *PFT*, 48); presumably it has no connection with *šukurum*,[17] since we would have to start thinking of an unicorn.

MISCELLANEOUS There is no milk (C1) at Persepolis, except perhaps in PF417. Many items are easier to recognise at Ashurnaṣirpal's feast than at Persepolis, but some of them may lurk there. The most obvious point of contact is sesame (C16) and its oil (C24, D9). Sesame is very common at Persepolis, and in fact there is no clear trace of oil except sesame-oil. Wherever ì.lg or *mil* is further qualified or elucidated, we seem to have sesame (PF431, 986, 1248). We still do not know the Elamite for 'salt'. Hallock (*PFT*, 25) suggested *madukka*, but this only appears in very small quantities, though often as royal provisions (PF719–22). If Ezra (vii. 22) could be given an unlimited quantity of salt, I doubt if receipts were issued at Persepolis for a quart. Hinz (1975: 83) preferred 'honey' for *madukka*; Hallock (*per epist.*) suggested 'coffee', without supporting it; is PF298 a coffee-cake? While we are on honey, note that C33 is explained by Diodorus, XVII.75.6; Curtius, VI.4.22. For C26–7 at the royal table, cf. Amyntas and Ctesias *ap.* Ath. II.68A. A10, D11: an article by M. Stol on cardamum suggests to me that it should be Elamite *zali* = Bab. *saḫlû vel sim.*[18]

DRINK The absence of beer is surprising, and it may have dropped out. The quantity seems very moderate, given Artystone's 200 m. The distinction drawn between palm-wine and grape-wine is of the greatest importance. Hallock offered no translation for the frequent *sawur* wine, Hinz came down for 'bitter' or 'herb' wine.[19] I put it to Hallock that it might be 'date-wine', and he could see no objection, but perhaps we ought to think about vinegar.

WEIGHTS AND MEASURES Apart from the shift over the *olyra* groats (A8, D5), the usage of dry measure, liquid measure and weights seems consistent and reasonable. It is less obvious why no category of foodstuff at Persepolis is weighed. The dry measure is the artaba. It is a good Persian measure, but only used at Persepolis in ways which await a full investigation (*PFT*, 72). An additional note defines the

[16] I. Gershevitch, *TPS* (1969), 169. [17] Hallock, *CDAFI* 8 (1978), 112.
[18] *JEOL* 28 (1983–4), 24–32. [19] *Neue Wege im Altpersischen* 83.

Median artaba as equal to an Attic medimnos, a very substantial overestimate. A smaller dry measure is the *kapetis*, defined as an Attic choinix, i.e. 1/48 of a medimnos; I know of no ancient oriental instance. As at Persepolis, the standard liquid measure is a *maris*, defined as 10 Attic choes, apparently another overestimate. The weight-measures, the talent and the mna, are always specified to be weights. It is easier to find estimates of capacity by waggon-loads in Greek (Xen. *Anab.* IV.7.10) than in oriental sources. However small the waggons, the chaff and straw (D7–8) seem ridiculously large quantities. ||

ANNOTATIONS That the equivalences of measures are editorial is clear enough, and, at the other extreme, the notes after C30, 33, 35, D11 seem to be part of the original. C19 is problematic.

33

Datis the Mede

Professor R. T. Hallock generously allows me to publish here his text and translation of the hitherto unpublished Persepolis Fortification Tablet Q–1809, to which he has referred in *CDAFI* 8 (1978), 115.

[1]7 mar-ri-iš [2]KAŠ.lg m.Da-ti-[3]ya gal-mā du-iš [4]hal-mi m.sunki-na [5]ku-iz h.Iš-par-da-mar [6]pir-ra-da-zi-iš [7]⌈iz⌉-zi-i[š] h. [8]Ba-ir-[ša] [9]m.sunki-⌈ik⌉-ka pa-raš [10]d.ITU.lg [11]Ša-mi-man-⌈taš⌉ [12]h.be-ul 27-um-[13]me-na h.Hi-da-li

[1–3]7 *marriš* beer Datiya received as rations. [4–5]He carried a sealed document of the king. [5–9]He went forth from Sardis (via) express (service), went to the king (at) Persepolis. [10–13]11th month, year 27. [13](At) Hidali.

On its left edge it bears Seal 201, which should belong to the supplier at Hidali. It appears in the same position there two months earlier on PF1404. Earlier seals for suppliers at Hidali are Seals 84 (this and not Seal 201 is on PF1408) and 138. The seal on the reverse is a small stamp, with figure at left facing an altar with an animal on it, with a moon above.

A beer or wine ration of 70 quarts marks its recipient as a very high personage. The figure recurs on PF1558 as the ration of Abbatema the Indian, who travels in considerable style (see Lewis, *Sparta and Persia* 5 with n. 14; his rations vary considerably, and he only gets 30 quarts in PF1556), and has been suggested as a likely ration for apparent sisters of the king (Hallock, *CDAFI* 8 (1978), 115). To our knowledge it is only exceeded by Parnaka, uncle of the king and chief economic official of Persis, who is on 90 quarts (PF664–5), and Gobryas, father of Mardonios, one of the greatest men of the kingdom, who is on 100 quarts (PF688). It is therefore very tempting indeed to identify Datiya with Datis the Mede, commander, along with the king's nephew, of the Persian expedition to Marathon in 490 (Her. VI.94.2). The

* Published in *JHS* 100 (1980), 194–5.

only objection seems to be the unassuming seal, but this may belong to a guide acting for him.

That a high official on a journey should be qualified with *pirradaziš* confirms Hallock's view (*Persepolis Fortification Tablets* 42) that the word is not a title in the strict sense, but may rather qualify the nature of the journey and the facilities to be extended to it. Since horses are also so qualified (PF1672.5, 1700.3, 2061.4, 2062.5, 2065.4), it may be that the special facilities involve special horses (cf. ||Lewis *Sparta and Persia* 57 n. 51). It has already been observed (*Persepolis Fortification Tablets* 42) that such journeys are either under royal authorisation (PF1285, 1320, 1321, 1329, 1335) or going to the king (PF1315, 1319, 2052); even in the apparent exception (PF1334) the messengers may be going to the king.

Though Datiya is going to the king, it is the king's authorisation he is carrying. Journeys are generally authorised from their point of origin (for apparent exceptions, see Lewis, *Sparta and Persia* 5 n. 14, 8 n. 31, and add the unpublished Q-901, 23rd year, 10th month, where Bakabana, based in Susa, authorises travellers from Sardis to Parnaka, probably only for the last section of their journey), and it may be that in normal circumstances only the king authorises return journeys (cf. perhaps PF1318, 1474, PFa31.13–15). It is at any rate clear on this occasion that Datiya has been on a round trip to Sardis and is now at Hidali, only three stages from the king in Persepolis, on his return journey. Hallock estimates this as at least four days' journey on foot, but he will have travelled faster.

The Persepolis tablets have rarely shed direct light on the highest politics, but this seems to be an exception. The date lies between 17 January and 15 February 494, in the winter before the closing campaign of the Ionian revolt. It was always likely that Datis had had some experience in the Ionian Revolt before his command against Eretria and Athens in 490, but clear evidence has been lacking. It now appears that he may have been sent by the king in person on a tour of inspection and coordination before the final campaign. Tithraustes' corresponding mission to Asia Minor in 396/5, with letters giving him the right to give orders to all satraps and the task of disposing of Tissaphernes, perhaps offers the closest parallel. No light is thrown on the question of how a Mede rose so high.

There is the unsolved question about the Lindian Temple Chronicle, which describes an undated attack by Datis on Rhodes (*FGrH* 532D). This is hard to fit into Herodotus' description of his movements in 490, and there is

some temptation to use it as evidence that he was fleet-commander in 494 (see, e.g., Burn, *Persia and the Greeks* 210, 218). All that can be said on the harder evidence of our tablet is that, if he reported to the king very quickly, he could have been back in the summer to command the fleet in the Aegean. Datis rapidly passed into Greek popular myth (see Raubitschek, in *Charites* [*E. Langlotz gewidmet*], 234–42), and it is the more gratifying to have him for once pinned down on a real occasion.

34

Persians in Herodotus

Toni was one of my first graduate teachers when I arrived in 1951 at what, after two years in the army, seemed only just this side of paradise, and he has been a steady support and solace ever since. It is the greatest possible pleasure to join in his birthday celebration.

This is not a wide-ranging investigation of Herodotus' interest in or attitude to the Persian Empire or the Persian character. The questions involved are about Herodotus the investigator and narrator, and are not new. They concern the methods by which he was able to know so much about that empire, and they involve the oldest questions of all: How much trouble did he take in the search for truth? Did he in fact have any conscience about the truth at all?

Historians are not unaware that Herodotus' truthfulness has been challenged from time to time, but on the whole they take no notice. To speak frankly, they have to ignore such criticisms or be put out of business, particularly when dealing with Persian history. Historians abhor a vacuum, and narrative sources on the Persian side are virtually non-existent. We only have the Babylonian texts which cover Cyrus' occupation of Babylon and the Behistun Inscription to cover the accession year of Darius. The result is that, if one is to write a narrative account of the reigns of Darius and Xerxes at all, there is || no alternative to using Herodotus' narrative as the core of that account. The assumption has to be that Herodotus is right, except when he can be shown to be wrong; that is a principle which is carried into many other aspects of near-eastern studies besides straight narrative. If you start with the postulate that you should use no Herodotus unless he can be shown to be right, you just won't get very far. That would be an inconvenient line, but not intellectually disreputable. It would be more disreputable to get

* Published in *The Greek Historians . . . A. E. Raubitschek* (1985), 101–17 (ANMA Libri and Co.).

caught, say, in the position of saying that Herodotus' account of Babylon is factually wrong, but must nevertheless be atmospherically accurate.

In this situation, any new opportunity for testing Herodotus' accuracy against outside sources must be taken, in the hope of determining where the probabilities lie for any given body of material. I think that such a new opportunity now exists, and I shall be exploring it in this paper.

That Herodotus had some very good sources about Persia has been clear since the decipherment of the Behistun Inscription. The main lines of his account of Darius' accession were clearly very similar to that of Behistun. Specifically, his list of the helpers of Darius in the assassination of the false Smerdis came out very well indeed. Unlike the epitome of Ktesias, who claimed to have been using official records, but only got one and a half names out of six right, Herodotus only got one wrong, naming Aspathines instead of, apparently, Ardumaniš. Even in naming Aspathines, he had some excuse, since Aspathines was unquestionably important later in Darius' reign, being named as quiver-bearer of the King on Darius' tomb, the only person besides Gobryas to be so named. The other prosopographical weakness is that he makes Otanes the son of Pharnaspes, and I can think of no way of making that correspond to Behistun's Θuxra.

It is clear of course that there is no question of Herodotus having used a written text of Behistun. But he might well have found one had he looked for it. Though the inscription itself was only in Old Persian, Elamite and Babylonian, there were other versions: §70 'Afterwards this inscription I sent off everywhere among the provinces. The people unitedly worked upon it.' If the Jews of Elephantine in southern Egypt had a copy in Aramaic at the end of the fifth century (this was a copy of an older manuscript), I would positively expect, considering the number of Darius' Greek-speaking subjects, that there was at some stage a Greek text, and I occasionally look hopefully at some of the scrappier late sixth-century Greek inscriptions of Asia || Minor in the hope of finding a fragment. But there is in fact no trace of Behistun having influenced a Greek historian, unless you include the unaccountable fact that Justin (1.9.7) calls one of the usurping Magi Cometes, which, considering the transmission-process, is not a bad shot at Gaumata.

Herodotus is broadly right on the names, and roughly right on the length of the usurpation. But he seems to have no knowledge of the claims to royal legitimacy which Darius puts forward and treats him as an ordinary Achaemenid. He gets various minor details wrong, like the place where the Magi were killed. Above all, he has virtually no knowledge, in this context, of the

extensive revolts which surrounded Darius' accession and which occupy so much of the Behistun Inscription.

His account of the accession, therefore, comes through an intermediary, and it has never played as prominent a part in the search for specific sources for his Persian information as three passages where the chances of a documentary basis have always seemed fairly high, the list of provinces or, rather, financial districts at III.89ff., the description of the Royal Road at V.52–4, and the account of Xerxes' army and its commanders at VII.61–98. These documents have not escaped criticism. The list of provinces has repeatedly been compared with lists issued by Darius and Xerxes themselves. Most of the discrepancies can be explained away if one thinks, with Cameron,[1] that their lists are simply lists of peoples, without administrative pretensions, but there are one or two awkwardnesses towards the east of the empire. There have always been some who have doubted whether the Royal Road really went that way, but, as far as I can judge, there is no real obstacle to believing that it describes at least part of the imperial road system. The list of Xerxes' forces is a more complex matter, which will be occupying us more and more in this lecture. The temptation has been felt to treat the whole list as a compilation from an ethnographic base, a literary invention to match the Homeric Catalogue of Ships, carrying the implication, which some have found incredible, that Xerxes was accompanied by men from all his peoples. I can say now that I shall eventually be accepting part of the case against this list, but that I find no serious difficulty in believing that the bare facts about contingents and commanders were available in || a documentary source. Such a list, it was claimed by the Alexander-historian Aristoboulos (Arr. *Anab.* III.II.3), was in fact found in the Persian camp after the battle of Gaugamela. If it turns out that we can increase our faith in at any rate the names of the commanders, we shall thereby be increasing our belief in Herodotus' ability to get reliable information. If we lose faith in these, the most documentary-looking, parts of Herodotus' account of Persia, the sceptics are fully entitled to their point of view.

Herodotus does give some indications of his sources, but even these may be doubted. Detlev Fehling, in a 1971 book[2] which is perhaps gaining in influence, attempted to establish the proposition that, in any case in which

[1] G. G. Cameron, *JNES* 32 (1973), 47ff.

[2] D. Fehling, *Die Quellenangaben bei Herodot* [translated 1989 as *Herodotus and his 'Sources'*].

Herodotus names his source, this is an infallible sign that he had no direct source. There is more to be said for this proposition than you may think, and its destructive effect is in practice modified by the fact that, although for Fehling the amount of invention in Herodotus is very considerable, he is willing to agree that Herodotus does not always make things up out of nothing at all. Despite Fehling's doubts, I should perhaps say something about what Herodotus says about his sources. By and large, he has certainly left us with the impression that his sources for Persian affairs are Persian. His account of Cyrus goes back to those Persians who do not want to exaggerate (I.95.1). That the Persians give two versions of why Darius' horse neighed (III.87) carries the implication that the narrative of Darius' accession is all Persian, and there is a similar allusion to what Persians say about Darius at III.89.3 just as the list of provinces starts. Thereafter we get remarkably little, but a continued attempt to suggest authentic Persian information comes out in a touch like 'Masistios, whom the Greeks call Makistios' (IX.20). Clearly all this does not add up to much, and, Fehling or no Fehling, the question of sources remains pretty open.

The search for Herodotus' sources for Persian affairs has been actively pursued, but it has mostly been conducted under the influence of a strange presupposition that there was a political and linguistic iron curtain between Greeks and Persians. It has therefore been thought necessary and desirable to look for very specific holes in this curtain through which Herodotus' information might have come. I do not think that Herodotus himself supports this attitude. We will recall that he lays stress on one point in Egyptian history where the || presence of Greeks starts (II.154.4): 'With these people settled in Egypt, the Greeks mixed with them and we know exactly everything which happened in Egypt starting with Psammetichus and later on.' This is not an excessive claim, and no one doubts the great superiority of Herodotus' coverage of the 26th Dynasty over his earlier Egyptian history. He makes no such claim for Persia, and the Greeks who first come into contact with Darius in his pages, Demokedes the doctor and Maiandrios the Samian exile, are there for their relevance to the development of Persian aggression against Greece. He draws no dividing line here about information, and I don't think that it occurred to him that he had to. The arrival of Maiandrios is surprising to Darius because he personally is new to the throne and no Greeks have yet called on him. There is, however, a staff of interpreters ready to elucidate the matter (III.140.2–3); there is no parallel

between this scene and the Assyrian texts of the 650s in which the messenger of Gyges of Lydia found no one at Assurbanipal's court who could understand his language.

Nevertheless, the tendency has been to look for the most obvious breaches of this supposed iron curtain, and some names have been fashionable from time to time. We do not hear much nowadays of the memoirs of Dikaios the Athenian exile (VIII.65). He at least has the merit of having told a story, but we do not know to whom he told it. Other guesses have only seemed plausible to their authors; the most recent[3] is the eunuch of Sataspes who ran away to Samos (IV.43 fin.). Some have had longer runs. There is quite a lot to be said for the exiled Spartan king Demaratos. At least we know that he had accessible descendants in the Kaikos valley, and there is even some reason to believe that Herodotus had been there (II.10.1). He is perhaps a rather more attractive source for dissensions in the Persian high command than for administrative detail. But the overwhelmingly favoured candidate has always been Zopyros son of Megabyxos, who, according to Herodotus himself, ran away from Persia to Athens at dates variously estimated as the late 440s or the early 420s.

I am not a Zopyros man myself and am sustained in that by my belief that the story for which he would be the most obvious source, his grandfather's services in the recapture of Babylon (III.150–60), appears on the face of it to be a pack of lies. This Babylonian revolt || is said to come very early in Darius' reign, but really the only point of resemblance between it and either of those of 521 described by the Behistun inscriptions is that it ends with 3,000 Babylonian nobles being impaled on stakes; the second Babylonian revolt in the Behistun inscription ends with nobles being impaled on stakes, the total of those killed in battle and surviving of the whole army being 2,497.[4] Such things must have happened not infrequently (Darius had impaled 49 the year before), and the coincidence does not seem to be sufficiently strong to outweigh the facts, (1) that it was Intaphernes whom the inscription credits with the capture of Babylon, (2) that the Babylonian

[3] D. Hegyi, *A. Ant. Hung.* 21 (1973), 73ff.

[4] Those who are most familiar with the Behistun inscription in its Old Persian version seem not to realise that the Babylonian and Aramaic versions add casualty figures. For the Babylonian version, see von Voigtlander, *The Bisitun Inscription of Darius the Great, Babylonian Version*. For the Aramaic version, see Greenfield and Porten, *The Bisitun Inscription of Darius the Great, Aramaic Version*.

revolts of 522/1 lasted two and a half and three months and not twenty, (3) that there is no Babylonian evidence that grandfather Zopyros was ever governor of Babylon, (4) that the kind of thing which Herodotus says about the fate of Babylon seems to fit much better what happened to Babylon in the early years of Xerxes (though even here we cannot fit in a siege of twenty months). In fact, this is the one place where they coincide, in which there is any temptation to prefer Ktesias to Herodotus. Ktesias does have a siege of Babylon early in Xerxes' reign. If Zopyros was Herodotus' source for all this, there is not much incentive to make him a source for the documents.

Let us start again. At some time not long after 493 a very large number of clay tablets was removed from the offices of the ration-administration at Persepolis and dumped in the construction of the fortification wall at the north-east corner of the Persepolis terrace. They did not reemerge until 1933–4 and, for excellent linguistic reasons, only one of them was published before 1969. We now have published texts of over 2,100,[5] relevant in various ways to the movement and disbursement of food stuffs in the area from 510 to 493. On grounds of scale alone, this is by far the most substantial accretion to our documentary knowledge of the Persian Empire in this century. They are not the only documentary texts from Persepolis. A || smaller group, about 130, of tablets similar in language and appearance was never dumped but remained in the treasury until Alexander came; they record disbursements of silver for a slightly later period, 492 to 458.[6] Many fields for investigation are thus opened, but I shall be trying today, on a fairly narrow front, to see what the finds do for our appreciation of Herodotus.

Two main points seem to emerge. The first is straightforward. We now have an enormous increase in our body of facts about the reigns of Darius and, to a lesser extent, that of Xerxes. As far as Herodotus is concerned, the preponderance of relevant facts is prosopographic, and this gives us a chance of making further tests on the extraordinarily rich material which he is able to produce about the Persian leadership.

The second is more indirect. The bulk of the tablets is in the isolated language of Elamite, heavily penetrated with Persian loan-words. Some of

[5] Hallock, *Persepolis Fortification Tablets*, from which individual texts are cited with the prefix PF, supplemented by *CDAFI* 8 (1978), 109ff., from which individual texts are cited with the prefix PFa.

[6] Cameron, *Persepolis Treasury Texts*, from which individual texts are cited with the prefix PT.

them also bore notes in Aramaic, no doubt made by those of the secretarial staff who are also described as Babylonian scribes, writers on parchment; there are some unpublished tablets in Aramaic as well. One tablet was in Phrygian. One of them, however, was in Greek, recording a transfer of wine. To simplify a long story, it results from the fact that there was at least one person in about 500 out on the administrative circuit to whom it came most naturally to write in Greek and who knew that there was someone in the central office who would be able to cope with it. The search for such persons is not a long one. No one has ever doubted that the Elamite word for Greeks is Yaunā-ip, and they are not infrequent in our documents, whether they are engaged in transporting building materials or employed more menially, like the twenty-three ladies who Hallock, the publisher of the tablets, thought were irrigation-workers and whom Hinz[7] more reasonably translates as 'spinsters'. (Since they are getting bonus rations for producing children, it should be clear that I use the word in its primary sense.) There are men in our texts who are simply called Yaunā. No one that I know of has spoken against the obvious view that this is not a true proper name, that the persons concerned are Greeks, known by their ethnics instead of their strange and no doubt unpronounceable names, just as the Greeks habitually called slaves Skythes or Kar. One is not all that high up, a grain-handler on an out-station in 503 and 502. The other two are. || One of them was from December 499 to September 498 the only visible aide-de-camp of Parnaka, the uncle of the King and chief economic official of Persis; the other, if he be another, is found early in 481 in the same position with the high official Artatakma. They are transmitting the high official's orders to the scribes who will actually write them down. They only dictate so they do not need to be literate themselves in Elamite; they do need to have enough spoken Persian and Elamite to do their jobs. On these facts, I argued a few years ago[8] that if we find Greeks in a secretarial capacity as early as this and as far east as this, there should be no reason to doubt their availability to the King, and to satraps, particularly in the west, in all relevant periods. None of my reviewers has objected and I hope that I can take this as reasonably established.

The consequence for today should be obvious. There was no iron curtain. It seems demonstrable that the Persian administration employed Greeks

[7] Hinz, *Neue Wege im Altpersischen* 95. [8] Lewis, *Sparta and Persia* 12–15.

at a level where certain kinds of information would be easily accessible to them, and this information could include general Persian information. Such people were not employed merely to write down the names of Greek trierarchs, though we certainly need them for Herodotus' list at VIII.85.2. There have been occasional references in the literature to the possibility of Herodotus' sources including them. They now seem to me to be very strong candidates indeed, and I think we have increased the chances of Herodotus having been able to acquire information.

That he could have done is not the same as saying that he did. To estimate the chances of that we need to look at the actual information which Persepolis provides. For those of my friends who may reasonably doubt the thoroughness of my knowledge of Old Persian and Elamite, I think I can say that I shall not be using many equivalences which have not also been used by Rudiger Schmitt[9] for purely onomastic purposes.

We have to start by understanding the nature of the evidence which the tablets are bringing to our knowledge of the Persian upper classes between 510 and 493. Some of the evidence is fairly straightforward. We can tell whether a person is important because of the range of || his interests and also if he receives unusually large rations of commodities. But the bulk of the evidence comes from the travel-documents. The Elamite form of the Old Persian for these is *miyatukka*, linguistically equivalent to the word *viaticum* which still exists today in a fairly specialised context. These are sealed documents ensuring the right of their holder to receive rations as he passes along the Royal Road. It is clear that the right to issue these documents is a function of the king or of someone at satrapal level; in two cases satraps seem to have deputies who can do it. By observing the direction of the journeys authorised, we can localise these satraps. The documents therefore enormously increase our knowledge of Darius' satraps, providing us with what looks like his uncle, Pharnakes the son of Arsames, mostly, though not always, in Persepolis, where he has a deputy Ziššawiš, a Megabanos satrap in Susa (and his deputy Mardunda), an Artabanos satrap in Bactria, Megabazos in Arachosia/Gandara/Parikania, Artabawa in India, Hydarnes in Media, and satraps in Areia and Carmania. A few other people are not so easy to locate. That list should indicate the limitations of our evidence. Persepolis is the furthest east of the Empire's capitals, and journeys to it are more likely to

[9] R. Schmitt, *A. Ant. Hung.* 24 (1976), 25ff.

come from eastern areas; even for Babylon we only have one journey to it and none from it; the stray people who come from Sardis are very unusual. Journeys from the western provinces are much more likely to end in Susa. This means that, since Herodotus' interests are inevitably fixed on the western side of the empire, the correlations are likely to be reduced. Correspondingly, the tablets give us people in the east of some importance who do not appear in Herodotus at all. This is a handicap, but working with the texts gives me reasonable assurance that most of the people in Herodotus must be real people. If one tries to correlate the names in Aeschylus or in Ktesias (for this period; he gets better later in the century), one gets a very sharp contrast and is left feeling that their names are merely vaguely oriental. A much lower proportion is directly translatable into Elamite or Persian.

I start with straight cases where Herodotus' information is confirmed.

(1) The most obvious one is that he makes Darius' full brother Artaphernes satrap of Sardis, where he stays, say, from 511 to 493. An Irdapirna issues travel warrants, one of which (PF1404) is explicitly for a journey from Sardis to Persepolis in November 495. ||

(2) In III.88 Darius marries Artystone, daughter of Cyrus. In the army-list at VII.69.2 further information is given about her. Her sons Arsames (*ibid.*) and Gobryas (72.2) are named as commanders, and she is said to be Darius' favourite wife. Irtašduna, twice qualified as *dukšiš*, appears on 25 Elamite texts, as a recipient of rations, as an owner of fairly small numbers of workers, and giving orders for the issue of provisions from her estate, from March 503 to some time in 497. The two earliest texts show the King authorising the issue to her of 100 sheep and 200 *marriš* (say 1,940 litres) of wine, perhaps for a special feast, and at least one more text (PF718) shows him closely concerned in her affairs. Three texts of 498 (PF733–4,2035) associate her with Iršama, who must be her son Arsames. Arsames in his turn (PF309, 498) orders the supply of grain to a woman called Uparmiya. She has been taken for his wife; there is more temptation to identify her with his stepmother, Parmis, daughter of Smerdis, another wife of Darius (III.88.3, VII.78).

(3) I come now to an older Gobryas. For Herodotus, he is, correctly, one of the Seven (III.70, 73, 78), and with Darius on the Scythian expedition (IV.132, 134). Thereafter he does not appear in person. Later references (VII.2.2, 97) tell us that Darius had been married to Gobryas' daughter

before he came to the throne and that Gobryas had married Darius' sister, by whom he had a son Mardonios (VII.5.1). Strictly speaking, Herodotus does not identify his Gobryases, but the Behistun inscription tells us that the Gobryas of the Seven was son of Mardonios, which is comforting. Mardonios makes his first appearance in 493 (VI.43.1) as commander in the West; he is young and has recently married Darius' daughter Artazostra. His campaign is a disaster; he loses his fleet off Mount Athos, is himself wounded in Thrace and goes off home. He is not employed by Darius on the Marathon campaign, which has new commanders, of whom more later. His next appearance is after Xerxes' accession (VII.5.1) where he is the man who has the greatest influence with Xerxes. He is consistently presented as the most zealous advocate of the invasion of Greece, as against the King's uncle Artabanos. He makes light of difficulties, tells lies about his own previous campaign (VII.9.2), and keeps on going, in 480 as the first-named of the six land-marshals (VII.82), in 479 as the principal commander.

There has not been any doubt that Herodotus has, if anything, underplayed the importance of Gobryas. After the murder of the false || Smerdis, he has a later command against Elam in the Behistun inscription, and, as spear-bearer of the King, is one of the only two Persians to be both portrayed and named on Darius' tomb. This position will account for the rapid promotion of Mardonios, though we note that it is not Artobarzanes, Gobryas' grandson (VII.2.2) but Xerxes who wins the succession to Darius. Artobarzanes turns up twice at Persepolis as a satrap, but we cannot locate him (PF1436, 2050).

The tablets help further. Kambarma (the Elamite form of Gobryas) turned up on them relatively early (PF688). In March 498 he was receiving a beer-ration of 100 quarts a day, 11 per cent higher than the next highest liquid ration known. It now appears (PFa5) that he was on that occasion meeting an unnamed lady, described as 'wife of Mardonios, daughter of the King'; she has an appropriately high flour-ration, the second highest known. The family party in that month is now completed by a lady Radušnamuya (PF684), with a wine-ration of 44 quarts a day; an unpublished tablet describes her as 'of Gobryas the . . .' Presumably she is his wife, Darius' daughter and Mardonios' mother.

Herodotus is thus confirmed on Mardonios' marriage, though not, apparently, on the chronology. For us, March 498 is not very recent in 493. I do not know whether this is a fact about his information or about his use of νεωστί,

a point which has been more warmly debated in connection with the career of Themistocles.

So much for the positive correlations. Negative correlations, of the type provided by Behistun, are not really to be expected; our documents are not like that. It would only be fair to say, since I have already said something about the great ladies, that it has proved distressingly difficult to find Atossa, daughter of Cyrus, wife of Darius, mother of Xerxes. There are other women about in the tablets, closely comparable to Artystone, above all Irdabama and Abbamuš, but it is not easy to make them into Atossa. The first thought was that she was dead before the tablets started, but Hinz, who suggested this, immediately withdrew it on the grounds that Aeschylus proved that she was alive in 480.[10] There is of course no evidence in Aeschylus' text that he knew the name or anything about her. I should attach more importance to Herodotus' belief (VII.3.4) that she was around || at the time that Darius designated Xerxes as his heir, but of course this is the sort of thing one could be agnostic about.

If you want negative correlations for Herodotus' oriental prosopography, you have to go outside the tablets and indeed outside the Persians. Straightforward reading of VII.98 and VIII.67 suggests that he thought that the King of Sidon in 480 was called Tetramnestos son of Anysos. Neither of these names looks very like Ešmunazar or Tabnit, which seem to be the relevant royal names in Sidon at the time.[11] Escape-routes have been tried and are available, but we should not start from the postulate that Herodotus must be right. A similar situation arises with the Lycian of VII.98, Κυβέρνισκος Σίκα or, better, Κύβερνις Κοσσίκα. There are coins with Κυ and Κυβ, but there are difficulties about the end of the name, and the more we know about the Lycian dynasty, the more awkward the middle of Κοσσίκα looks.[12]

I pass now to points where the tablets provide information consistent with Herodotus in a way which makes his information sound more plausible. The first is rather picturesque. Darius, at the start of his reign, wishes to remove Oroites the satrap of Sardis, and this is ingeniously achieved by Bagaios the son of Artontes (III.127–8). This ought to get a suitable reward, but Herodotus says nothing. You will remember that Artystone was labelled

[10] *Orientalia* 39 (1970), 423; *ZAssyr* 61 (1971), 291.

[11] Galling, *ZDPV* 79 (1963), 150 n.51; Dunand, *MelUSJ* 49 (1975/6), 495.

[12] J. Bousquet, *CRAI* (1975), 142 n. 12.

dukšiš. This ought to mean something like 'princess', and that has been recently confirmed by its appearance (PFa31.13) qualifying ladies named as daughters of Hystaspes, who ought to be sisters of Darius; they have enormous beer-rations. One other *dukšiš* remains, Ištin (PF823), who receives two sheep together with Bakeya, who I assume is her husband. That ought to be Bagaios. In other lands one gets half the kingdom as well as a princess for notable service, but that is hardly possible in Persia. At the risk of running ahead of my chronology, I draw attention to Mardontes son of Bagaios, commander of Red Sea islanders in 480 (VII.80) and one of three fleet-commanders in 479 (VIII.130.2, IX.102). This may well be the same Bagaios, since grandson and grandfather will share a name-element, and there is a very suitable identification with Mardunda, deputy satrap in Susa in 499–494.

With one prominent exception, which I defer, no very plausible identifications appear for most of Darius' commanders in the west, || though most of the names can be readily rendered into Elamite. Nothing makes the identification of Daurises, Hymaies and Otanes (V.116) with people of the same name more than possible. There is the occasional possibility of reversing the process. An isolated high personage, who cannot be placed geographically, called Ziššamakka (PF1493; December 500) was treated by Hallock as a by-form of the well-known Ziššawiš. This was unnecessary. Consider the Sisimakes (Sisamakes, Sisamankes, Susamankes) who gets killed in high command, say in 497 (V.121).

I have already published[13] a contribution to the history of the Ionian Revolt. Herodotus has no knowledge of the career of Datis before his appointment to command the Marathon campaign of 490, though he knows that he is a Mede. It had been guessed that he ought to have had some previous experience before his collaboration with Darius' young nephew Artaphernes, and a stray reference in the Lindian Temple Chronicle has been used to support a guess that he was a commander in 494, a year in which Herodotus gives no fleet commander. Publishing a tablet concerning one Datiya with the high beer-ration of 70 quarts, I argued that this was Datis, on his way back from Sardis in January 494 from a mission to report to Darius on the Ionian Revolt.

I now turn to the names in the catalogue of Xerxes' army. By now, we are thirteen years after the last Fortification Tablet, but it is clearly not unlikely

[13] Lewis, *JHS* 100 (1980), 194–5 = this volume, 342–4.

that at least the senior people in 480 will be visible at Persepolis in some capacity.

Starting from the top, the six marshals of VII.82, we begin with three of Xerxes' cousins, Mardonios, Tritantaichmes and Smerdomenes. Mardonios we have already met, but no new light is thrown on Tritantaichmes or Smerdomenes, or indeed on the fourth marshal, Masistes, Xerxes' full brother. The fifth marshal, Gergis son of Ariazos or Arizos, has always been enigmatic, with no visible high connections to fit him for his grand company. Burn[14] suggests that he may be the indispensable master of detail, the adjutant- and quartermaster-general, although he does not visibly behave as such, and finds it interesting that someone from outside the Seven Houses should rise so high. It is therefore comforting to be able to suggest an identification. Travel-authorisations give us a Karkiš, satrap of || Carmania at least from 501 to 494, and that looks quite a plausible 'praetorian' province for an able man on his way up in a career based on talent. There is nothing implausible about his father's name. It will correspond to Elamite Harriyazza which comes twice, though not for high personages. The sixth marshal is Megabyxos son of Zopyros, safely attributable to one of the Seven Houses. He has recently been claimed[15] as *the* great Persian military innovator. Surviving into the 440s, he will be on the young side in 480. Persepolis only produces an undated 'companion of Bagapukša' on a slightly abnormal ration-scale (PF1255).

On this level we can put the commander of the Immortals, Hydarnes son of Hydarnes (VII.83); whether he also held a coastal command in Asia and when (VII.135), need not detain us. He clearly should be the son of Hydarnes of the Seven; that *he* was satrap of Media until at least 499, I have argued elsewhere.[16] There is another possible son, commander of the Arians in VII.66.1, Sisamnes son of Hydarnes, but the name is not uncommon, at various levels.

I pass now to the hipparchs (VII.88). One of them, Pharnuches, had an accident and was left behind, which left Harmamithres and Tithaios, sons of Datis. Herodotus does not say that this is *the* Datis, but it is clearly not unlikely. Datis was a Mede, Media was a horse-rearing country, and some have found him taking a special interest in cavalry on the Marathon campaign. I can offer nothing on Harmamithres beyond the fact that

[14] Burn, *Persia and the Greeks* 323. [15] P. A. Rahe, *AJP* 101 (1980), 88–9.
[16] Lewis, *Sparta and Persia* 84 n. 14.

Harma- is a Persian name-element, but I do find some interest in Tithaios. Old Persian roots in Ciça, Ciθra produce Elamite forms in Zišša, and a whole range of Greek equivalents Τισσα, Τριτα, Τιθρα, Τιθα, Σισ. So Tithaios ought to come out as Elamite Zišša*ya* or something like it. There is no difficulty in finding a candidate here. The single most frequent character in the Persepolis tablets is Zišša*wiš*, spelt in fourteen different ways, and the nearest Greek equivalent to that is certainly Τιθαῖος. Zišša*wiš* is a principal functionary at Persepolis from August 504 to February 467. Seldom quite at the top, he is nevertheless substantial. He uses a seal of Darius himself. We have no evidence for his rations after 499, but even then, when he must be quite young, he is on 1½ sheep, 60 quarts of flour and 30 of wine a day. I do not speculate on the || dynasty's use of Medes, but this seems a suitable career for an important one.

Now the admirals (VII.97). These start with Ariabignes, Xerxes' half-brother, and Achaimenes, his full brother. Neither of these has yet turned up at Persepolis. The third is Prexaspes son of Aspathines. I said before that Aspathines was not one of the Seven, although Herodotus thought that he was, but that Herodotus was partly excused by the fact that Aspathines was eventually very important with Darius, appearing on his tomb as quiver-bearer. At Persepolis Aspathines appears late in the Fortification Tablets in 494 and early in the Treasury Tablets in 483. He is the most obvious successor, if we look for a single one, for the King's uncle Parnaka as Zišša*wiš*' superior in charge of Persepolis. The inscription on his seal has been read[17] as 'son of Prexaspes'. If this is right, this would make the connection back to the Prexaspes who plays a somewhat equivocal role in Herodotus' third book, but gets a good press for it, and forward to this admiral. Even if it is not right, there is clearly sufficient oriental evidence to put a son of Aspathines at this sort of level. The fourth admiral is Megabazos son of Megabates. The names are confusing, but Burn[18] has attempted to sort out Herodotus' data. I can contribute what may be another Megabazos (Bakabaduš), satrap in Arachosia and Gandara in 501 to 494, but the attraction of making him a parallel to Gergis is somewhat reduced by the thought that a career in Afghanistan and Baluchistan is not a very satisfactory preparation for being an admiral. I wonder whether the job is not in some sense hereditary. After all, a Megabates is a fairly prominent admiral around 500 (V.32ff.). Some

[17] Cameron, *Persepolis Treasury Texts* 104. [18] Burn, *Persia and the Greeks* 335.

time late in the reign of Darius he still seems to be described at Persepolis as 'the admiral', Bakabada *MÀ gi-ul-li-ra* (PT8).[19]

It would be intensely tedious to go down a level, to the commanders of contingents in any detail. I have noted one or two already, and the general impression of reliability remains. To take one name in particular, for his importance in 479 rather than in 480, Artabazos son of Pharnakes, singled out by Burn[20] as a rare commoner, now || falls nicely into place as son of Pharnakes-Parnaka, Darius' uncle, so prominent at Persepolis and unknown to Greek sources. He would certainly have the social position to argue with Mardonios.

I confess that it is hard to avoid conducting such an investigation without tacit assumptions that Herodotus should be thought right and that the task is to fit other information round him, but, by and large, this does not seem to be an impossible task in this case. I do not see much alternative to the view that there is at least some nucleus, documentary or quasi-documentary, to Herodotus' list, which correlated contingents with real names. There have been many to say that it is impossible that Xerxes should have taken such a motley collection with him, a view strongly combated by Burn.[21] There have been complaints about the difficulties of correlating the national contingents with oriental information, a view most strongly put recently by Armayor.[22] But Armayor has not, at least in print, attacked the commanders' names, and I do not readily see how this could be done.

I do not think that what stands as Xerxes' army-list is a unitary document. It has always been relatively easy to detach the ethnographic notes about the former names of various peoples, particularly those which attempt to tie them into Greek mythology, and most of them have close points of contact with other parts of Herodotus' work,[23] though he does not always tell precisely the same story; Herodotus would be perfectly capable of doing this amount of embroidery for himself. More trouble arises when we consider the weaponry attributed to the national contingents. Here I have considerable sympathy with Armayor, though not all his arguments are equally strong. It is true that Schmidt, in commenting on the Persepolis reliefs,[24]

[19] I should note that two late sources (Diod. Sic. XI.12.2; Strabo IX.2.9, p. 404), had Megabates himself as admiral-in-chief in 480.

[20] Burn, *Persia and the Greeks* 324. [21] Burn, *Persia and the Greeks* 325–6.

[22] O. K. Armayor, *TAPA* 108 (1978), 1–9. [23] VII.93 makes an explicit reference to I.171.

[24] Schmidt, *Persepolis* iii: *The Royal Tombs and Other Monuments* 143ff.

is an extreme case of credulity about Herodotus, tying himself into considerable knots rather than discard Herodotean information. I am satisfied that there is a strong case, as Armayor indicates, for believing that much of this weaponry came from the ethnographic work of Herodotus' predecessor Hecataeus and not from any official list. To restate the point more fully than Armayor has yet done in print, it is at least || a remarkable coincidence that the two points where we know what Hecataeus said about people's costumes find their exact reflection in Herodotus. Hecataeus (*FGrH* 1F287) said that the Matienoi wore the same costumes as the Paphlagonians; so does Herodotus (VII.72.1). Hecataeus (1F284) said that the Kissians wore Persian clothing; so does Herodotus (VII.62.2). And there is a further point which I find convincing. At VII.77.1 Herodotus is talking about the Kabelees and says 'they had the same equipment as the Cilicians which I shall describe when I get to the Cilicians as I go through'. It seems to me that the reasonable inference from this sentence is that Herodotus is taking his information from a written source which had described the Cilicians before the Kabelees, and adapting it to his own purposes which involve a list which has the Cilicians after the Kabelees.

I concede therefore that there is a good deal in the list of Xerxes' army which is not documentary, but this is not the same thing as conceding that there is no documentary core at all. In fact, this may be the single case where it is legitimate to try to unwind Herodotus and disentangle what he gives us into separate strands. It would be gross folly to attempt to extend this in any detail into non-documentary passages where Herodotus has woven his material more closely, but I do not think that we should neglect the general indications provided by this particular case about the way in which he has built up his work and formed first-hand information and the work of his predecessors into a literary narrative.

I come back finally to the documentary core. What kind of list was it and who gave it to Herodotus? I think that there is one more point to be made. There was not simply a list, but someone who transmitted it, embroidering it as he went along with a little extra detail and explanation, about, for example, who was married to whom, who was an Achaemenid, if that was not obvious, and scraps about later careers. We get such extra information for 11 of the 29 contingent commanders. One of these bits (VII.69.2), the longest, is that most amply confirmed by Persepolis, and such extra details

are provided for the high command as well. My guess remains that this informant was a Greek. It would however be fair to say that there is a chronological gap before Herodotus can have talked to anybody, and I would not say even now that it was illegitimate to speculate about a written source. I shall not join in.

35

The Phoenician fleet in 411

Tissaphernes' first set of instructions from Darius,[1] to bring in or kill Amorges (Thuc. VIII.5.5), were carried out by the end of the summer of 412 (VIII.28). To do this, he had used Spartan help against Athens, Amorges' ally (VIII.54.3; And. III.29), and had committed the King by treaty to make war jointly with the Spartans (VIII.18). All this was presumably covered by his instructions, but may not have been worked out in detail at Susa, since in early winter 412 he is still claiming that he will need to refer to the King before he pays the Spartans a full drachma a day (VIII.29.1, 45.6).

The results of his report to the King on the operations of 412 emerge next spring in the third treaty with the Spartans (VIII.58). This is made before the end of Thucydides' winter, but after 29 March 411, since it is in Darius' thirteenth year.[2] The Spartans will only be paid for their operations until the arrival of the King's fleet; if they still want money after that, they will have to repay it at the end of the war. When the King's fleet arrives, it will fight the war at the side of the Spartans and their allies, in whatever way Tissaphernes, the Spartans and their allies think best. The fleet, therefore, has been mobilised in Phoenicia towards the end of the winter, on the King's instructions (see also VIII.87.5), and is on its way by the spring (VIII.59). There is no clear account of its arrival at Aspendos, but it may be thought of as there by June, 147 in number (VIII.87.3).

At Aspendos it comes under Tissaphernes' orders. What general instructions he may have received about its use must remain unknown.

* Published in *Historia* 7 (1958), 392–7.

[1] My thanks are due to Professor A. Andrewes for stimulus and discussion, to Professor J. Czerny and Mr J. R. Harris for note 14, and to Professor G. R. Driver for his help with the Aramaic and the willingness with which he has connived at this revision of his views.

[2] My dates are all from the second edition of Parker and Dubberstein, *Babylonian Chronology* (1946). [There are no relevant changes in the third edition (1956).]

Certainly Thucydides in his exhaustive discussion of the problem (VIII.87) speaks as though Tissaphernes had a completely free hand to use it if he wished; why it was not used, is for him a problem about Tissaphernes' motives, and not a very difficult one. In any case, by the end of Thucydides' narrative, the fleet still seems to be at Aspendos, although Alcibiades claims to have averted the possibility of its ever being used (VIII.108.1). Thucydides' view is that its failure to appear is purely due to Tissaphernes' wish to wear down both sides (VIII.87.4). ||

Hatzfeld[3] has wished, correctly, I think, to adduce wider considerations, for which there is one piece of Greek evidence. Xenophon never mentions the fleet at all, but there is a certain amount of material in Diodorus. We may ignore the confusion caused by Diodorus' attribution of everything done by either satrap to Pharnabazos; Tissaphernes does not appear at all in Book XIII. The first reference is in XIII.37.4–5. Alcibiades persuades Pharnabazos to send the '300' ships back to Phoenicia and to adopt the policy of wearing down both sides, and the ships go. In XIII.38.5 this news reaches Mindaros, who gives up hope of help from Pharnabazos, and goes on with his own operations, which include the sending of Dorieus with 13 ships to Rhodes, a fact which is certainly true[4] and notoriously omitted by Thucydides. XIII.41.4 reproduces, very roughly, Thuc. VIII.108.1, Alcibiades' return from Aspendos, and 42.4 covers some of the same ground as Thuc. VIII.108.4–5, adding the information that the Spartans were annoyed because the ships had been sent back to Phoenicia. It is clear that, for the source of Diodorus, the fleet has already gone back to Phoenicia before the end of Thucydides' narrative, not merely that the fleet was not coming further than Aspendos, which is all that Thucydides implies (VIII.99, 108.1).

There is one further reference. At the time of the battle of Abydos, Pharnabazos explains to the Spartans that he had sent the ships back to Phoenicia πυνθανόμενος τόν τε τῶν Ἀράβων βασιλέα καὶ τὸν τῶν Αἰγυπτίων ἐπιβουλεύειν τοῖς περὶ Φοινίκην πράγμασιν (XIII.46.6). Only Pharnabazos can be on the spot. Diodorus has him apologising for something which must have been done by Tissaphernes, and we can hardly unravel the muddle or deliberate conflation. The apology is however worth scrutiny.[5]

[3] *Alcibiade* 251–3. [4] Xen. *Hell.* 1.1.2.

[5] It has only really had it from Hatzfeld's source, Mallet, *Rapports des Grecs avec l'Égypte* 82. Mallet seems to me to take it too literally; he believes in the possibility of an actual

There is no other literary reference to an Egyptian, let alone an Arabian, revolt at this time. Kienitz[6] describes Diodorus' statement as an *ungeheure Übertreibung*, which might however indicate the existence, otherwise unknown, of an independent dynast in the Western Delta in 411. The only Egyptian trouble he recognises in this period is in the next year, 410, when local Egyptian priests, with the co-operation of a Persian commander, attacked and destroyed the Jewish temple at Elephantine.[7] This episode he shows to have been an extremely local affair, without any wider anti-Persian significance.[8] ||

There is now a good deal more evidence not available to Kienitz. But even Cowley no. 27 may perhaps imply more than he realises. Starting with its second complete sentence, it describes the sacking of the Temple at Elephantine, of which nos. 30 and 31 give us a fuller account and the exact date, Tammuz of the 14th year of Darius (14 July–12 August 410). This second sentence begins 'In the 14th year of King Darius, when our Lord Arsames went away to the King . . .' and it would seem that this begins the description of the matter complained of. The previous sentence ought to refer to something else. It runs 'When detachments (?) of the Egyptians revolted, we did not leave our posts and nothing disloyal was found in us.'[9] This can hardly refer to what follows, an attack on themselves, made with the support of a local Persian commander; it must be the end of a general description of the loyalty of the Jewish garrison of Elephantine, giving the reasons why they feel themselves entitled to support in this crisis. At some time before 410, then, Egyptian detachments had revolted.[10]

raid on Phoenicia. Since his dates for Amyrtaeus are impossible and he does not have the new Aramaic evidence, his detailed account no longer stands.

[6] *Politische Geschichte Ägyptens* 73.

[7] Cowley, *Aramaic Papyri of the Fifth Century B.C.* nos. 27, 30, 31.

[8] Drioton and Vandier, *L'Égypte* 605–6, take a contrary view, without good grounds.

[9] This is Cowley's translation. Professor Driver tells me that there is no reason for doubting the reading of the word translated 'detachments', but that there is no parallel for *bên*, translated by Cowley 'when', as a conjunction. He would prefer, therefore, 'amongst detachments of Egyptians (which) revolted, we did not forsake our posts', for the relative particle may be omitted if the antecedent is indeterminate.

[10] Professor Driver confirms me in this interpretation and would modify his paraphrase (p. 91, abridged edition) accordingly. The same conclusion has been arrived at by Kraeling, *Brooklyn Aramaic Papyri* 103, who suggests the accession of Darius II as a suitable time for this revolt. There is no other evidence and it would be surprising and complex to have an Egyptian revolt at the same time as the satrap of Egypt is himself revolting in the interests of Darius (Ctesias 47).

The new evidence is more explicit. The new Aramaic documents published by Driver[11] have three references to an Egyptian revolt. They are all in letters from Arsames, not in Egypt, but in Babylon or Susa. No. v deals with the fate of certain slaves of Arsames who, 'appointed . . . to my domains, which are in Upper and Lower Egypt, when Egypt revolted and the troop was held in barracks, . . . did not succeed in entering the fortress. Afterwards the reprobate Anu-dārû (?) seized them, (and) they were with him.' They are now prisoners, but Arsames gives orders for their release and return to his service. In no. VII Arsames' officer in Lower Egypt is not showing the same care as his predecessor or Arsames' servants in Upper Egypt[12] in dealing with his estates, which seem to have suffered 'when the Egyptians revolted'. In no. VIII Arsames deals with a claim by an Egyptian forester to succeed to his father's farm, which was abandoned after the death of his father and all the women of the house, 'when the rebellion occurred in Egypt'. (Since VIII, || like VII, is addressed to Neḥtiḥur, I take it that this farm was in Lower Egypt.)

Unfortunately, the Driver dossier is undated. To modify his statement of the dating in accordance with the chronology of Parker and Dubberstein, he places it between spring 410 (since Arsames' departure seems from Cowley 27.2, 30.4–5, 31.4, to fall in the 14th year of Darius, i.e. after 17 April 410, and before the Elephantine episode of July 410) and late 407–early 406 (since Arsames seems to be back in Egypt at the time of Cowley 32, which is a reply to Cowley 30–31, of 26 November 407; see Cowley 30.30). Driver's reasons boil down to two; the documents are written during the absence of Arsames from Egypt, and he certainly was absent from summer 410, and the rebellion in Egypt is to be identified with the destruction of the Temple at Elephantine. This last view will hardly stand. As we have seen, the opening lines of Cowley 27 seem to distinguish an Egyptian revolt from the events of 410, and the very full narrative of Cowley 30 shows no signs of the Elephantine episode being part of a wider revolt. In fact Cowley 30.30 states that Arsames knew nothing of the Elephantine episode. It seems impossible to fit the events of 410 into operations which could be described as an Egyptian revolt, and which, as we see from Driver V–VII–VIII, covered both Lower and Upper Egypt.

[11] *Aramaic Documents of the Fifth Century B.C.*

[12] Driver following Kutscher reads 'Lower Egypt', but only the last two letters are certain, and sense and spacing seem to me to make necessary the restoration of the word for 'Upper Egypt'.

Cazelles[13] has considered and rejected the possibility of dating the Driver dossier late in the decade 460–450. The two points in favour are the identification of the fortress of Driver v with the White Fort of Thucydides, 1.104.2, which clearly bears no weight at all, and the appearance of the name Anudārû in Driver v. The *daleth* in this name is a restoration, and Driver suggests that the name is Elamite or Accadian. This has little historical probability, and I have every sympathy with the wish of Henning and Kahle[14] to see here a form of Inaros. But, leaving aside the question of whether Inaros is a prophylactic or a patriotic name (and, if patriotic, it might, I suppose, be taken as a *nom-de-guerre* by any rebel leader),[15] there is no improbability in a second Inaros in a period where there is a second Amyrtaeus. Nor does the Arsames || evidence really square with this theory. The appearances of Arsames in the Cowley collection as (presumably) satrap of Egypt run from 428 (?) (Cowley 17) to 407/6 (Cowley 32), and he is attested in that post in 424 under the name of Arxanes by Ctesias 47–8. Babylonian documents referring to him run from 423 to 404,[16] with the exception of one, which may belong to 463, but which is much more likely to refer to 403. The only evidence which really might push his satrapy back above 450 is Ctesias 35, where Megabyzos appoints Sarsamas or Sartamas[17] satrap of Egypt after the capture of Inaros. Ctesias, despite his faults, generally sticks to the same name for the same person, but our evidence is so scanty that we cannot absolutely bar the possibility of having Arsames as satrap in the late fifties. I would, I think, rest my rejection of the attribution of the dossier to those years on its general atmosphere, which seems more

[13] *Syria* 32 (1955), 97–9.

[14] *Theologische Rundschau* N.F. 17 (1948–9), 207–8. This preliminary reference to the dossier takes the early dating for granted.

[15] Spiegelberg (*Rec. Trav. Eg. Assyr.* 28 (1906), 197–201) argued that the translation of the name should be 'The eye of Horus is against them' and thought of it as a name protecting its bearer against his enemies, without making it clear whether these enemies are to be thought of as spiritual or material. Mlle. Guentch-Ogloueff (*BIFAO* 40 (1941), 117–33) has assembled a large collection of similar names, and translates this one as 'May the eye of Horus be against them'. She suggests that the object of the curse is a foreign invader, and regards all such names as patriotic names. Her conclusions are approved by Ranke, *Die aegyptischen Personennamen* ii. 224, but really form no more than an untested hypothesis. In any case, there is no question of the name being rare or confined to one family.

[16] Driver, *Aramaic Documents*, Appendix.

[17] I take the MSS readings from the edition of R. Henry, Brussels, 1947.

appropriate to a period after a short revolt soon crushed than to a period after ten years or so of revolt, with resistance still going on in the West. If we accept this early date, we still have the first sentence of Cowley 27, and have to choose between the slight improbability of the Jews calling attention to their loyalty of forty years before and hypothesising a separate revolt to explain it.

The rejection of the early date does not automatically entail the date of 410–407 for the dossier. Arsames may have gone up to the King on other occasions earlier than 410. The attraction of the date lies in the Diodorus passage. In the Driver dossier Arsames is away from Egypt, and there has been a recent Egyptian revolt. We know from the Cowley papyri that Arsames went away from Egypt in 410 and from Diodorus that there was some talk of trouble in Egypt in 411. The gaps in our knowledge are wide, but the coincidence is attractive. It seems that a revolt in 411 is the most likely hypothesis at the moment. The only fact that may have to be posited to support it is the appearance of a second Inaros.

The course of events seems to be this. In winter 412/11 the Phoenician fleet is mobilised and put under Tissaphernes at Aspendos. How far-reaching his instructions were, we shall never know. Before Tissaphernes made up his mind to use the fleet (not to exclude the possibility that he made up his mind not to use it), a revolt broke out both in Lower and in Upper Egypt, perhaps under a second Inaros. This seemed potentially so dangerous that the fleet was removed from Tissaphernes' command and returned to Phoenicia, where it might be needed in operations against the rebels. Alternatively, but perhaps rather less probably, Tissaphernes decided that he did not want the fleet, but used the Egyptian revolt as a plausible excuse for the Spartans. The revolt was however stamped out by the spring of 410, and, although many administrative matters remained to be dealt with, the military situation was || quiet enough for Arsames to be able to leave his satrapy in early summer 410 and to go to Susa to report. In his absence we have the purely local affair at Elephantine in July, but this has no anti-Persian tinge, and even though Amyrtaeus counted his regnal years from 405/4,[18] he may not have been causing the Persians very much trouble even then. The Persians are still in control at Elephantine in 402.[19]

[18] Kienitz, 178; Drioton and Vandier, 622.

[19] Or, less probably, 401. Kraeling, *Brooklyn Aramaic Papyri* no. 12.

This seems a consistent picture. It was too easy for the Greeks and still is for Greek historians to conceive the Persian King and his subordinates as having nothing to think about except Aegean problems. If we take this narrow view, the indecisive nature of Persian intervention in the Aegean from 412 to 407 is perhaps mysterious. But the King has more on his mind than Greece. I would not like to assess the date or the importance of the palace troubles of Ctesias 84–7,[20] but events in Egypt and, probably much more important, in Media, where a revolt did not end until 408/7[21] and had to be followed by a campaign against the Kadousioi,[22] provide reasons which may have caused the delay of Persia's revenge on Athens.[23] The failure of Persia to throw in her full weight before 407 was not due to lack of will, but to more pressing business.

[20] For an uncritical account, see Olmstead, *History of the Persian Empire* 363–4.

[21] Xen. *Hell.* I.ii.19. I would attribute this paragraph to the interpolator. It therefore belongs, wherever it comes from, to his date (408/7), not the true date (409/8) of the events that Xenophon has been describing.

[22] Xen. *Hell.* II.i.13.

[23] I am not denying that Tissaphernes' policy suited Tissaphernes personally. It could not have been operated by Cyrus. But I give Darius credit for being able to choose his instruments. Once conditions were ripe for active participation, Cyrus was admirably fitted for operating the new policy.

36

Persian gold in Greek international relations

No Greek had any doubt that the King of Persia was enormously wealthy. Twelve hundred camels brought him his money, says Demosthenes (XIV.27), in comparing his resources with what fourth-century Athens might produce. The majority of papers for this round-table are concerned with the details of this wealth. I shall be trying to explore some of the ways in which it was used in relation to Greek affairs.

In a famous passage (III.89.3), Herodotus tells us that before Darius there was no regular tribute in Persia; the subjects gave gifts. It was Darius' institution of regular tribute which earned him the designation *kapelos*. Even in the elaborate tribute-list which came to Herodotus (III.89–97), there is a section devoted to those nations, the Ethiopians, the Colchians and the Arabs, who remained on a nominal gift structure. I do not propose to discuss this in detail, and I shall not be asking whether there was really no tribute in some areas before Darius, whether Herodotus was right in asserting that Persis paid no tribute, whether Herodotus' list represents satrapies or financial districts, or whether it really belongs to the reign of Darius rather than that of, say, Artaxerxes I. My first point, rather, involves the exploration of gifts. ||

Although it did not attract the attention of Marcel Mauss, the master of the subject, the Achaemenid Empire in fact constitutes a textbook case of a gift-centred economy.[1] Gifts were exchanged on a regular basis. Even after the introduction of tribute, it is fairly obvious that individuals and communities continued to make presents to the King of enormous quantity

* Published in *REA* 91 (1989), 227–34.

[1] For material and discussion on gifts in Persian society, see Knauth and Nadjmabadi, *Das altiranische Fürstenideal von Xenophon bis Ferdausi* 189–95, Sancisi-Weerdenburg, *Yaunā en Persai* 145–83. N. Cahill, *AJA* 89 (1985), 373–89, argues that the Persepolis Treasury was primarily used to receive gifts, but only down to the late fifth century.

and variety. I can slide over individual references by reminding you of the Persepolis reliefs of present-bearers, generally held to be arriving on New Year's Day, and one literary reference, Theopompus *FGrH* 115F263, the great list of the varied objects brought to the King as gifts. A visit from the King was the occasion for a lavish and crippling display of hospitality, particularly since, as was not understood by Greeks, the King and his companions assumed that the gold and silver tableware was part of what was being offered (Her. VII.118–20, Theopompus F113).

On the other side, it was inherent in the King's position that he had to make presents as well, and we have a certain amount of scattered information about fixed occasions for his doing so, notably on the anniversary of his accession (Her. IX.110.2, cf. III.1) and on such rare occasions as the promotion of a Crown Prince (Plut. *Art.* 26.5). On these occasions, we are told that he cannot refuse a specific request, and there are other occasions (e.g. Her. IX.109.2–3) on which a King gets himself into trouble by rash promises. I have a suspicion that it may be a Persian habit to avoid even receiving specific requests. Correspondingly, it seems to be expected that the King's gifts should be willingly received (Her. IX.111.5).

It is not surprising that we find the King making presents for good service on all sorts of occasions. In the campaign of 480, we find presents for the best equipped contingent (Her. VII.26.2), a reward for the first captain to capture an Athenian ship (Her. VIII.10.3), and more *ad hoc* rewards for individuals and cities who render good service (Her. VII.106.1, 116, VIII.120). Even the ship's captain who had got Xerxes into danger gets a gold crown for saving the King's life before having his head cut off (Her. VIII.118.4). Such presents can be found with great frequency down to the end of the Empire. I single out one of the latest: the rewards for Mentor for his services in the reconquest of Egypt in 343 (Diod. Sic. XVI.52.2). His *aristeia*[2] include a hundred talents and the best of expensive *kataskeue* as well as high command.

Gifts are also a necessary concomitant of hospitality, and our best evidence, which also happens to be most relevant to our subject, concerns foreign ambassadors.[3] There was probably a regular scale (cf. τῶν νομιζομένων in Plut. *Pel.* 30.7), but this might be exceeded in the case of those who

[2] The word is now attested twice in inscriptions about presents from Egyptian kings to foreign mercenaries; see C. Ampolo and E. Bresciani, *EVO* II (1988), 239.

[3] For some early documentary evidence about the treatment of a distinguished foreigner, see Lewis, *Sparta and Persia* 5–6.

particularly found favour with the King, like the record-holder, obscure to us, Entimos of Gortyn (Phainias *ap.* Ath. 11.48D–F) and Timagoras the Athenian ambassador of 367, who (Plut. *Art.* 22.9–12, *Pel.* 30.9–12) got 10,000 darics, 80 cows to provide milk for his illness, a *kline, stromata* (and people to make the bed); the king was even said to have paid his || litter-bearers four talents. Other gifts became famous: Pyrilampes the Athenian got peacocks;[4] Antalkidas the Spartan, less expensively, a rose drenched in perfume, certainly a special favour, but made fun of by Greek moralists.[5]

There is no reason to doubt that virtually all the royal behaviour I have so far been describing (and it can be detected at lower levels as well) fits into a pattern which the King and Persians in general would have regarded as normal behaviour. I am not trying to idealise Persian behaviour. I am not denying that gift-exchanges within the Empire may sometimes have involved a *quid pro quo* of a type which we might be inclined to consider improper, but it is in fact actually rather hard to find, within the Persian Empire, cases of actual bribery. For the pre-Herodotean period, I so far only see the bribing of the judge Sandokes (Her. VII.194.1).

As against this pattern of attitudes, we have to set some rather different Greek attitudes. For Greeks, the one word δῶρον has to cover both 'gift' and 'bribe'. Improper use of *dora* goes back at least to the δωροφάγοι βασιλῆες of Hesiod (*Op.* 221, 264) who pervert justice, and Herodotus, on the Greek side, is full of people, particularly, it has been noticed, Spartans,[6] who will change their minds or their actions for money. When a Greek sees a *doron*, his inclination is to suspect it, and it is incumbent on the recipient to make sure that his motives are pure. The classic statement is that of Hyperides (*C. Dem.* 24–6): the Athenians give orators and generals the right to benefit themselves, on condition that what is taken is for the benefit of the city, not against it. That Athenian politicians took money was regarded as normal, even if it was better to be like Pericles (Thuc. 11.65.8) and not take it.

There are therefore transactions which may look different from the Persian side and the Greek side, and the Persians may have had to get used to what seemed to them odd Greek behaviour. To confine ourselves to embassies for the moment, the main context of Theopompus' account of

[4] Forthcoming articles by M. C. Miller and P. Cartledge explore this gift and its later history. [See Miller, *Arch. News* 15 (1989), 1–10; Cartledge, in Cartledge *et al.* (eds.), *Nomos* 41–61.]

[5] Lewis, *Sparta and Persia* 147. [6] Finley, *The Use and Abuse of History* 167 with n. 11.

gifts to the King is embassies from the whole of Asia. Although some Greeks had once understood the importance of gifts in diplomatic negotiations with Orientals (the Spartans to Croesus, Her. 1.70.1, III.47.1.), I cannot see a single occasion on which any Greek state outside the empire thinks it necessary to send a present to the King of Persia. Generosity here is entirely a one-way relationship. That ambassadors were lavishly treated was known and often the subject of fun on the stage and in the assembly (Ar. *Ach.* 61–90; Plut. *Pel.* 30.9–12), but this lavish treatment could always be turned into a political charge. Timagoras the milk-drinker was executed when he got home, though not apparently only because of the milk (cf. Xen. *Hell.* VII.1.38). Those who had principles, or who were worried about what might be thought at home, were more cautious about what they took. It was also in 367 that Pelopidas (Plut. *Pel.* 30.7–8), though offered very notable gifts, took nothing except a symbol of *charis* and *philophrosyne*; Antiochos the Arcadian pancratiast refused to take anything at all, and made caustic remarks about Persian luxury. I suspect that such refusals were thought strange or rude by their hosts; parallels suggest themselves from the history of British India or the rules of various diplomatic services about the receipt of presents.

We should, I think, accept that, in the early Achaemenid period, the Persian attitude to gifts was relatively straightforward, that they were either matters of routine or rewards for services actually rendered. The key point, and here I touch on ground already covered by Dr Lombardo,[7] is that a reading of the first eight books of Herodotus seems to reveal no Persian case of money being used as a weapon of war. They never bribe anyone to commit treachery to their own state. || No suggestion that this is possible is made until 479, and then it comes (Her. IX.2.3) from their Greek allies, the Thebans. Mardonius will get all he wants without trouble if he sends money to the most powerful men in the cities: this will create *stasiotai* for him and split Greece apart. Mardonius does not listen. Even when his colleague Artabazus adopts the Theban line (IX.41.2), pointing to all the gold and silver, coined, uncoined and in drinking vessels, which he has with him and which he can use for this purpose, he still insists in fighting a battle in the true Persian way, νόμῳ τῷ Περσέων, and meets with disaster and death.[8]

The earliest hint that the lesson had been learned may come in Xerxes'

[7] [M. Lombardo, *REA* 91 (1989), 197–208.]

[8] Nevertheless, when Lykides (Her. IX.5.2) advocates listening to Mourychides, Herodotus does consider it possible that he has received money from Mardonius.

negotiations with Pausanias; he urges him to spare no expense in his service (Thuc. 1.129.3), but there is no statement that any money was ever sent. Better evidence comes from the year 456 or thereabouts, when Artaxerxes I sent Megabazos, presumably a fairly high-ranking Persian, to persuade the Spartans to invade Attica in order to draw the Athenians away from Egypt. Though some money was used, the mission failed (Thuc. 1.109.2–3). Diodorus (XI.74.5–6) says that the Spartans refused. There are intractable nuances here. As a new book suggests,[9] 'The cash was not necessarily a direct bribe: rather, the intent was to defray expenses.' Possibly connected with this is the case of Arthmios of Zelea (Dem. IX.41–4, XIX.271–2, etc.), who 'brought gold to the Peloponnese' at some undefined date during the Delian League period.[10]

Some of us have argued that the 'First Peloponnesian War', to which Megabazos's mission was addressed, was not a war to which the Spartans were deeply committed.[11] When the Peloponnesian War proper started, the Peloponnesian League was totally outweighted financially by Athenian resources, and it is natural that the idea of getting financial help from Persia should arise first on the Spartan side. It is suggested by Archidamos (Thuc. 1.82.1), and various later efforts were made (Thuc. II.7.1, 67 [with Her. VII.137], IV.50[12]). But the Athenians apparently took up the idea pretty fast (Thuc. II.7.1); Pericles' optimism about Athens' financial reserves took some pretty heavy knocks from 432 to 428. The possibility of getting Persian gold is well established by Aristophanes' *Acharnians* of 425, and there has evidently been some actual contact. Spartan contacts are broken off in 424. Though the Athenian embassy in that year didn't get very far (Thuc. IV.50), there is a strong case for supposing that they went again not much later, though no case for assuming that any money passed. I have argued[13] that neither side had much to offer Persia at this stage.

I have introduced most of our principal themes. What Greek states want

[9] McGregor, *The Athenians and their Empire* 58.

[10] Meiggs, *The Athenian Empire* 508–12, argues for a connection with Pausanias, but his arguments against the normal connection with Megabazos depend: (1) on the decree's really having been proposed by Cimon; (2) on a belief that Demosthenes had a precise knowledge of the circumstances. The sceptical discussion by C. Habicht, *Hermes* 89 (1961), 18–19, 23–5, obscures the fact that Demosthenes was drawing on an inscribed text.

[11] A. J. Holladay, *JHS* 97 (1977), 54–63; D. M. Lewis, in *Classical Contributions . . . M. F. McGregor* 70–8 = this volume, 9–21.

[12] Lewis, *Sparta and Persia* 63–4. [13] *Ibid.* 65–6.

is financial support, particularly when involved in wars against financially superior opponents. Greek individuals may simply want money, although of course it may be true that they want it to promote policies in which they believe. What the King wants above all after 479 is to clear or protect the Asiatic mainland. This may involve supporting a particular Greek state and || subsidising its war effort. It is also a theme after 401 that he needs Greek mercenaries for campaigns in his own sphere, and this is sometimes presented as a reason for his wanting peace in Greece; his subjects in revolt have similar motives.

Our fullest relevant narratives cover the period from 412 to 387.[14] After the Athenian disaster in Sicily, there was now a real possibility of loosening Athens' grip on the coast of Asia and the initiative came from the Persian satraps, Tissaphernes and Pharnabazos, under pressure from the King to recover full control on the mainland, and they were ready to invest money in this cause. Tissaphernes (Thuc. VIII.5.5) merely promised *trophe* for a Spartan fleet. Pharnabazos' envoys (Thuc. VIII.6.1, 8.1) actually had 25 talents with them. The naval campaigning which would be necessary was beyond the financial resources of the Spartans, so that there were good prospects of striking a deal. I have discussed elsewhere the complications of Tissaphernes' relations with Sparta, including their financial aspects. What is in question is the amount and nature of Persian pay for the fleet. Tissaphernes' promises were not worth much. After negotiating a reduction in rates of pay (VIII.29) to, perhaps, 33 talents a month for the Spartan fleet then present, he still paid very sparingly and, eventually, not at all (e.g. 46.5, 57.2, 78, 80.1, 83.2–3, 85.3, 87.1–3, 99). It may be that he was following the advice of Alkibiades (VIII.46.2) to let both sides wear each other out, but it is also evidently true that he was operating entirely with his own financial resources; it can be suggested that things will be better if money ever comes from the King (VIII.45.5–6). There are suggestions (VIII.38.4, 39.2, 45.3, 50.3, 83.3) that money is passing from Tissaphernes to Astyochos the Spartan nauarch and other allied commanders as individuals to induce them to moderate their demands for pay for their men. Speculation about his parsimony[15] extended to the idea (VIII.87.3) that he hoped to make money out of

[14] Most of what follows is touched on in one way or another in Lewis, *Sparta and Persia*; I see little point in giving detailed references.

[15] But he can alternatively be represented as capable of being so enthusiastic in the Athenian cause that he will coin his own throne if necessary (VIII.81.3).

the Phoenicians by summoning their fleet to the war area and then letting them go. I do not know whether any Persian nobleman would contemplate such a course of action.

The Spartans eventually transferred themselves to Pharnabazos, who always enjoyed a much better relationship with Greeks. There were no complaints about pay that we hear; that the Peloponnesian fleet was much reduced by naval defeat in this period may be relevant. He was capable of grand gestures which have nothing to do with his own war. When Hermokrates wanted to go back to Syracuse, he gave him, without being asked,[16] enough money to build five triremes and hire a thousand mercenaries (Xen. *Hell.* I.1.31, Diod. Sic. XIII.63.2). It is clear that Pharnabazos got through a great deal of money; it should be remembered that both he and Tissaphernes had troops of their own to maintain.

The situation was transformed by the appointment of Cyrus to the western command in 407. For the first time, the King brought his own financial resources to bear. Cyrus came originally (Xen. *Hell.* I.5.3) with 500 talents, and assured the Spartan Lysander that, if this ran out, he would use the *idia* which his father had given him. If even these were exhausted, he will mint his gold and silver throne.[17] They asked for a drachma a day, but are told that this is contrary to Darius' instructions; the treaty said (Xen. *Hell.* I.5.4–7) 3,000 dr. a month for as many ships as the Spartans wish to maintain. Lysander got him up to four obols (which in fact || is roughly a daric a month). At this rate, 500 talents would pay the seventy ships Lysander had in 407 for about eleven months, but the number of ships will have risen. Against this, Lysander's successor Kallikratidas had difficulty in getting paid. The original royal grant had run out by 405 (II.1.11) and much more had been used. I have guessed that Cyrus was empowered to use the tribute of his extended satrapy as well, even before he got on to the private resources he envisages using. The only later hard figure is that the surplus left over at the end of the war from Cyrus' φόροι amounted to 470 talents in silver (Xen. *Hell.* II.3.8, cf. II.1.14). Precise computations are hard to make, but I do not think that the assertions by Athenian orators that the King spent in all 5,000 talents (Andoc. III.29; even more Isocr. VIII.97) are to be taken seriously, even if we include the expenditure of the satraps.

[16] By contrast, Tissaphernes at VIII.85.3 asserts that Hermokrates had once asked *him* for money and had become hostile to him when he refused it.

[17] Compare n. 15 for this motif.

After the end of the war, the most important event financially was Cyrus' revolt against his brother in 401, which involved collecting the large mercenary force which we know as the Ten Thousand. Persians in revolt had been using Greek mercenaries for at least twenty years and must have had means of paying them, particularly since it was rapidly found that they could be bribed to change their allegiance (Ctesias, 50, 52). We should not forget this type of impact on the monetary system.

As far as the international scene was concerned, the main effect of Cyrus' revolt was to break the diplomatic link between Persia and Sparta. Whatever concessions Sparta may have made about the Greeks of the mainland were revoked and for five years Spartan forces in Ionia had come to replace the fifth-century Athenian presence. Their removal become the prime objective of Persian policy. Since the land forces available were incapable of doing the job, the two weapons which were available both involved the use of money.

The first was a naval rearmament programme, using the Athenian admiral Konon. It was started in 397 by Pharnabazos with 500 talents of silver, presumably from the King (Diod. Sic. XIV.39.1). Quite a lot of this must have gone on preliminary equipment, and how much was available for pay is not clear. What is clear is that the fleet was extremely badly paid. In 395, we are told (*Hell. Oxy.* 19.2) that the fleet was owed pay for many months (fifteen according to Isocr. IV.142); we shall discuss the reason given later. The matter was temporarily alleviated, not by a fresh royal grant, but by the execution of Tissaphernes. The visiting commissioner Tithraustes ordered that 220 talents of silver from Tissaphernes' property be used for pay for the fleet, and left the royal generals with about 700 talents for the war, apparently from the same source. This is the nearest we ever get to estimating the size of the funds of a satrap; I very much doubt whether Tissaphernes had been anything like as wealthy in 412/11. Trouble over pay seems to have recurred, and it was not until Konon went to Babylon and made a personal approach to the King that he secured the appointment of a *tamias* to give him 'as much money as he might request' (Diod. Sic. XIV.81.5–6). There is no further trace of money trouble. Konon won the battle of Knidos in 394, and in the next year Pharnabazos sails to Greece with him and provides subsidies for the anti-Spartan coalition and the rebuilding of the Athenian Long Walls (Xen. *Hell.* IV.8.8–10, 12 and the muddled Diod. Sic. XIV.84.5).

In the middle of this sequence belongs the other, very famous, use of

money as a weapon. The threat posed by Agesilaus' major campaign in Asia was met along the lines suggested by the Thebans and Artabazos in 479. The primary text is Xen. *Hell.* III.5.1, where in 396/5 Timokrates the Rhodian is given by Tithraustes gold to the amount of fifty talents in silver to secure assurances from the leaders in the cities of mainland Greece that they would start war against the Spartans. It is likely (*Hell. Oxy.* 7.5 (again χρυσίον), Polyaen. 1.48.3) that this is wrong in date and that Timokrates came from Pharnabazos; a mission of this kind would be particularly likely || to suffer from the distorting effects of rumour; yet another version (Plut. *Art.* 20.4) says that Timokrates was sent by the King. War did break out on the mainland, not necessarily simply for this reason, and Agesilaus was summoned home, saying, in a famous pun, that he had been driven out of Asia by 30,000 archers (Plut. *Art.* 20.6, *Apophth. Lac.* 211B (golden darics), whence *Ages.* 15.8 μυρίοις is emended). At 20 dr. to the daric, this would come not to fifty talents, but a hundred. This is probably distorted rumour again, but *Hell. Oxy.* 18.1 suggests that Timokrates had said that more money would be available.

I do not see another clear case of money being used to influence the actions of Greek politicians. The nearest case might be in the 'King's gold' (βασιλικὸν χρυσίον) which Demosthenes is said to have received in about 336 to impede the start of Alexander's invasion of Asia.[18] By the year 330, Aeschines was asserting a figure of 300 talents for this. It is clear enough that some at least of this money was used and meant to be used for supporting the revolt of Thebes in 335. Estimates of how much Demosthenes kept for himself therefore vary, and the transaction cannot be described as purely 'political'. In the next few years, Menon was given royal money to bribe Greeks to the Persian side (Diod. Sic. XVII.29.1, 4), and Agis III of Sparta (Diod. Sic. XVII.48.2) got ships and money from the King. At this point, the Empire itself is in danger, but there had been earlier direct financial support for Greek states, notably the gift of 300 talents to the Thebans in 351 (Diod. Sic. XVI.40.1–2). That was a response to a direct Theban request, as they struggled against the enormous resources which the Phocians were drawing from the treasures of Delphi.

Within the King's own sphere, we get a certain amount of fourth-century

[18] Schaefer, *Demosthenes und seine Zeit* iii². 115–18, Davies, *Athenian Propertied Families* 134.

detail about the financing of expeditions, which certainly involved paying for Greek mercenaries. The largest single figure for any royal expenditure is the 2,000 talents which Tiribazos was given for the Cypriote campaign of the 380s (Diod. Sic. XV.4.2).

Much expenditure on Greek affairs, which it would be tedious to detail, comes from satraps, loyal or disloyal, and not from the King himself. Despite the King's wealth, we sometimes get the impression that he is extremely reluctant to use it. Thucydides, in a rare reference to the King (VIII.87.5), has a suggestion that the King may prefer economical policies, and there is a famous and powerful passage of the *Hellenica Oxyrhynchia* (19.2) in connection with the shortness of pay for Konon's fleet: 'for they were paid badly by their commanders, which is always the case for those who fight on the King's behalf. Even in the Decelean War, when they were allies of the Spartans, money was provided very meanly and sparingly, and the allied triremes would often have become unserviceable, had it not been for Cyrus's zeal. For this the King is to blame. When he starts a war, he sends the commanders a little money at the beginning and pays little attention for the rest of the time, and those in actual charge, if they do not have their own money to spend, sometimes see their forces falling apart.'

After his list of tributes, Herodotus (III.96.2) describes how the King melts it down and stores it, until he needs it. It is certain that Alexander found vast unused resources in Persian palaces, preserved untouched by the Kings as a refuge against the vicissitudes of Fortune (Diod. Sic. XVII.66.2). M. de Callataÿ's paper gives more detail on this.[19] Altheim and Stiehl[20] have elaborate calculations to show that the average proportion of tribute put to reserve was about || 5 per cent; my suspicion is that this is rather low. I do not think we should think of the King as simply being mean. We are rather moving in a world where there was no attempt to form a coherent financial policy. The King is more likely to be moved by specific requests to make his gifts, and the importance of a personal approach is often recognised.

Quantification is impossible, but I get the impression that the amount of money passing from Persia into any purely Greek area was not very large, and it may not have affected the money supply very greatly. For this purpose, we have no evidence but the gold–silver ratio. I have argued elsewhere[21] that

[19] [F. de Callataÿ, *REA* 91 (1989), 259–74.]

[20] Altheim and Stiehl, *Die aramäische Sprache unter den Achaimeniden* i. 120–37.

[21] D. M. Lewis, in *Essays in Greek Coinage Presented to Stanley Robinson* 105–10.

this became lower in the last quarter of the fifth century, but I see no reason to suppose that Persian gold was a particularly substantial factor in this. Although my title speaks of Persian gold and the word *chrysion* is prominent in the sources from the *Acharnians* to Aeschines, numismatists will readily recognise that a great deal of Persian expenditure must actually have been in silver. Since mainland Greece remained almost entirely silver-based, further speculation would depend on assembling a range of prices and wages; the evidence is insufficient. I only observe that, if the transmitted figures for the amount of Delphic treasure the Phocians used (Diod. Sic. XVI.56.6) are even approximately correct, this must have had a more substantial effect on the money supply than any Persian expenditure. Since this is also the period of increased Macedonian mining activity, the picture is inevitably confused.

37

The first Greek Jew

Dr M. Mitsos publishes in Ἀρχ. Ἐφ. (1952), (published 1955), 194ff., an inscription which may be of interest to a wider public. It is from the Amphiareion of Oropos, the small border state between Athens and Boeotia which continually changed hands between them and sometimes had a precarious independence. The Amphiareion, dedicated to the healing-hero Amphiaraos, was of more than local importance, and attracted pilgrims and dedications from all Attica and Boeotia.

Μόσχον Φρ[υνίδας . . .]
τον καὶ εἶναι ἐλεύθερον μη[δενὶ μηδ]ὲν
προσήκοντα· ἐὰν δέ τι πάθηι Φρυνίδας
προτοῦ τὸγ χρόνον διεξελθεῖν, ἐλεύθερος
ἀπίτω Μόσχος, οὗ ἂν αὐτὸς βούληται.
Τύχηι ἀγαθῆι. Μάρτυρες· Ἀθηνόδωρος
Μνασικῶντος Ὠρώπιος, Βίοττος Εὐδίκου
Ἀθηναῖος, Χαρῖνος Ἀντιχάρμου Ἀθηναῖος,
Ἀθηνάδης Ἐπιγόνου Ὠρώπιος, Ἵππων Αἰσχύ-
λου Ὠρώπιος. Μόσχος Μοσχίωνος Ἰουδαῖος
ἐνύπνιον ἰδὼν προστάξαντος τοῦ θεοῦ
Ἀμφιαράου καὶ τῆς Ὑγιείας, καθὰ συνέταξε
ὁ Ἀμφιάραος καὶ ἡ Ὑγίεια ἐν στήληι γράψαντα
ἀναθεῖναι πρὸς τῶι βωμῶι.

[. . . Phrynidas (will release) Moschos to be free, dependent on no man. But if anything happens to Phrynidas (i.e. if he dies) before the time elapses, let Moschos go free wherever he wishes. To Good Fortune. Witnesses: Athenodoros son of Mnasikon of Oropos, Biottos son of Eudikos of Athens,

* Published in *Journal of Semitic Studies* 2 (1957), 264–6. (Reprinted by permission of Oxford University Press.)

Charinos son of Anticharmos of Athens, Athenades son of Epigonos of Oropos, Hippon son of Aeschylos of Oropos. (Set up by) Moschos son of Moschion the Jew at the command of the god Amphiaraos and the goddess Health, having seen a dream in which Amphiaraos and Health commanded him to write it on stone and set it up by the altar.]

First, the date. This can only be deduced from the letter-forms, but the amount of comparative material available is so great that Mitsos' date of 300–250 BC is quite certain. This makes Moschos by quite a considerable margin the earliest Jew known from || mainland Greece, and he replaces in this position the slave simply named Ἰουδαῖος who was freed at Delphi in 163/2 BC.[1] He is well established in the Greek world. Not only does he have a pure Greek name (meaning 'calf'), but so does his father.

He is the slave of a certain Phrynidas, probably a Boeotian, to judge from the name, and the inscription begins, as we have it, in the middle of a typical manumission formula. Full freedom is to be given him at the end of a term of years, with the proviso that, if his master dies before the term is up, the freedom takes effect immediately.

But none of this is as remarkable as the fact of the inscription being carved on marble at all and the reasons given for it. At Delphi manumission inscriptions exist by the hundred. This is the very first which we possess from the Amphiareion, despite the large number of inscriptions which have been found there. This is certainly something abnormal, and Moschos makes a special point of explaining his reasons. The gods of the place have appeared to him in a dream and told him to do so. If this is all there is to it, Moschos would still be at least remarkably receptive to Greek ideas. But all that we know of the Amphiareion makes it probable that he took a more active interest in them. It seems to me nearly certain that he had gone to lie in the temple for a night in search of advice or a cure and had received the dream there. We are well informed about the ritual from an inscription.[2] A sacrifice was necessary, the priest took a fee and kept a visitors' book, visitors were many and the sexes strictly segregated. Moschos, the first Greek Jew of whom we know, was thoroughly assimilated to his Greek environment.

The date of his appearance in Greece and the fact that his father and he

[1] Frey, *Corpus Inscriptionum Judaicarum* (*CIJ*) 710. The date of *CIJ* 709, the Jewish slave-woman and her two daughters, is now known to be 158/7 BC.

[2] *SIG*³ 1004.

have Greek names make it necessary to revise quite considerably our ideas about the first arrival of Jewish slaves on the mainland of Europe. The latest treatment known to me, that of Miss B. D. Mazur,[3] tends to treat it as a result of the activities of Antiochus Epiphanes and their aftermath, but even the evidence available to her should have made this doubtful, since *CIJ* 709 (158/7 BC) has a mother Antigona beside her two daughters who bear the typical Greco-Jewish theophoric names || of Theodora and Dorothea, and it seems likely that her family had been in captivity for some time for her to acquire such a name. Our new evidence makes it likely that we should be prepared to reckon with Jewish slaves in mainland Greece at any time after the invasion of Palestine by Alexander. The stage between them and the Jewish communities in Sparta and Sicyon in 139[4] remains obscure. It is unlikely that Moschos had much communal Jewish spirit.

[3] *Studies on Jewry in Greece* i. 8. [4] I Macc. 15.23.

38

Review of *Do You Know Greek?*

How Much Greek could the First Jewish Christians have Known?

by J. N. Sevenster

The scope of this careful, sensible, and up-to-date book is adequately indicated by its sub-title. It deserves wide circulation and consideration. It has been admirably translated by Mrs J. de Bruin, who deserves a special word of praise.

An introduction (pp. 1–21) sketches the reasons why the problem is important and concentrates inevitably on the Epistle of James and the prominence of the linguistic argument in discussions of it. The author detects a regrettable subjectivity in approaches to James and enunciates clearly the need for objective criteria.

The work proper begins with a three-part chapter on Literary Sources, covering the New Testament (pp. 23–38), the Rabbinical Writings (pp. 38–61), and Josephus (pp. 61–76). We are thus confronted immediately with what seems to me the book's most obvious and regrettable omission, a section on the inter-testamental period. To put my requirements at a minimum, we needed a discussion of the Septuagint Esther which declares itself clearly to have been translated in Jerusalem. Most of us would go a stage further and assert that at least B and E of the additional material were written rather than translated there. Whether the Septuagint Esther is a product native to Jerusalem or whether Lysimachos son of Ptolemy was a settler from Alexandria does not seem to me to make much difference in considering the availability of reasonably good Greek in Hasmonean Jerusalem. Clearly the way in which the Hasmoneans communicated with foreign

* Published in *JTS* n.s. 20 (1969), 583–8. (Reprinted by permission of Oxford University Press.)

states and the Diaspora also needs consideration. The investigation will start from II Macc. 1, but will not end there.

The New Testament section starts with a discussion of the absence of references to language difficulties and interpreting, which seems to slide over problems about the nature of the Gospel narrative in that it will be evaluated differently, depending on the reader's view of where any particular passage stands in the spectrum between factual report and story-telling. I see no difficulty in the author's conclusion that Jesus may well have been able to conduct simple Greek conversations, but one might, for example, find oneself believing that Alexander the Great spoke and understood some very peculiar languages, if one used too rigidly the test that no interpreters are mentioned. A sensible discussion of Acts 6.1 follows, with the conclusion that a difference of vernacular language is indicated, and the section ends with a short discussion of the relative roles of Hebrew and Aramaic in the period.

The Rabbinical section opens with the question of loan-words, rightly despairs of the state of their detailed investigation, and endorses Lieberman's view that they constitute evidence of considerable linguistic penetration at a relatively early period. The familiar Talmudic direct references to Greek are considered, with due regard to problems of chronology and whether dicta are relevant to Palestine or the diaspora. Curiously, the author discusses Gittin ix.8 without reference to the direct reference to the practice of Jerusalem contained in it, even || though he holds that it is practically certain that the chapter has Palestine in mind; the cautious may wish to maintain that careful people in Jerusalem were only careful about the use of patronymics, not about the placing of Greek signatures. Sotah ix.14, on the abolition of Greek teaching, is rightly dated to the War of Quietus rather than to the War of Titus, though without the reference to the Seder Olam which could have shortened the discussion of the reading. The author is surely right to suppose that even the later dating of the ruling does not make first-century Greek teaching much less likely. The Talmudic texts which link on to this concern the House of Gamaliel. The author's admirable caution is well illustrated here, since he is much less inclined than Lieberman to give full credit to the five hundred young men in Gamaliel's house who studied Greek Wisdom, and stresses the length of the chain of evidence behind the statement. What, incidentally, should we think of as constituting Greek Wisdom in Gamaliel's period? I have a nasty feeling that it is

more likely to be a rhetorical training than anything else, and this would rather be confirmed by the fact that the house of Gamaliel was allowed to teach it because of its relation with the government. Later rabbis may have thought of philosophical instruction, and it is natural that we should, but is it really likely? The author tackles the question of whether Homer is referred to in the Talmud, not very conclusively, and eventually stresses the chronological span involved and comes back to the relatively hard fact that, if teaching Greek was forbidden in AD 118, its practice must go some way back before that.

The Josephus section starts with a frank admission that there are clear indications in *BJ* that many Jews were speaking their mother-tongue, whichever that might have been, and that interpreting was necessary. The author toys, I fear, at some length with a disastrous suggestion that interpreting was necessary because Titus' Greek was bad, and spends a page or so on Augustus having been less fluent in Greek than Claudius and that the same might have been true for Titus. A reference to Suetonius, *Titus* 3, 'Latine Graeceque, vel in orando vel in fingendis poematibus, promptus et facilis ad extemporalitatem usque', would have saved him this one. The *BJ* passages must be given their full weight, and I doubt whether even v.361 means that Titus went down personally and shouted in Greek, just out of range, like Josephus in the next sentence. There follows a long discussion of the implications of *Ant.* xx.262–5, from two main points of view. Firstly, it is argued that the lack of favour among the Jews for τοὺς πολλῶν ἐθνῶν διάλεκτον ἐκμαθόντας καὶ γλαφυρότητι λέξεων τὸν λόγον ἐπικομψεύοντας διὰ τὸ κοινὸν εἶναι νομίζειν τὸ ἐπιτήδευμα τοῦτο μόνον οὐκ ἐλευθέροις τοῖς τυχοῦσιν ἀλλὰ καὶ || τῶν οἰκετῶν τοῖς θέλουσι 'imparts an important piece of information about his people's knowledge of foreign languages, in this case Greek: if he had a mind to it, every freeman, even a slave, could achieve much as regards that knowledge, even as regards style and pronunciation. Evidently Josephus assumed that every inhabitant of his country could learn to speak quite good Greek if he were at all desirous to do so. This demonstrates that Greek was not only spoken in a few groups and classes, but that everyone in the Jewish country had the chance of speaking it. Greek could evidently be heard in all circles of Jewish society. And it was considered quite a common thing that all sorts of people from all sorts of classes became very proficient in speaking it.' This is a crucial argument for Sevenster, recurring on his very last page, and seems to me to be making far too much of

the passage, particularly of τοῖς τυχοῦσιν. I cannot see that the passage says more than that linguistic proficiency can be acquired irrespective of social class. It does not seem to me to have to be about Jews alone. No one will doubt that there would be ample opportunity in Judaea to see and hear of polyglots and even of slaves trained to considerable linguistic proficiency. Our own experience suggests that it is better to start young, a point made neither by Josephus nor by Sevenster when he comes to discuss the second main point arising from the passage, Josephus' own sufferings with and proficiency in Greek. Here I am in general agreement with his interpretation. Even Josephus didn't find Greek easy.

Chapter 2, 'Archaeological Material', is far longer (pp. 77–175) and provides the main justification for a new book on the subject, since much new material has accumulated since Lieberman. It certainly constitutes a valuable guide to the material, but I am not entirely happy about its topographical organisation. Sevenster makes the chronology fairly clear when it can be made clear, but we would have been helped if he had found some way of indicating how the evidence was distributed over the area in different periods, and we are given a good deal which has no possible bearing on the first century AD. I will spare the reader detailed niggling, and pick out some main points. There is no possible doubt on the prevalence of Greek in the Diaspora and on its probable importance for knowledge of Greek among inhabitants of Palestine who had contacts with Diaspora Jews. Hellenistic and Herodian towns must have made some knowledge of Greek inevitable for many inhabitants of their neighbourhood and more for those who lived in them. I agree with Sevenster that it is scarcely conceivable that rural Galilee remained hermetically sealed off from the penetration of the Greek language, though I have had some experiences with elderly Welsh country-folk which would qualify the picture rather. Sevenster rightly || says that visitors for festivals would perform the same function for Judaea. I am slightly doubtful about arguing from government documents set up for public exhibition and this has special application to Galilee. (The Froehner plaque on tomb-robbing is now supplemented by Seleucid documents two centuries earlier, *IEJ* 16 (1966), 54–70.) Governments' ideas on what ought to be readable do not necessarily coincide with what the subjects can actually read, and much the same applies to coins. For Jerusalem, the Ophel synagogue inscription and the ossuaries are decisive for widespread Greek in

the first century, and Sevenster rightly argues that there is little reason to believe that many of the latter belong to Diaspora Jews.

For Galilee, the necropolis of Beth Shearim occupies a good deal of Sevenster's attention, although *Beth She'arim*, ii. *The Greek Inscriptions* came too late for him to use. The segregation of the Greek inscriptions in that volume, and the confining even of the Hebrew/Aramaic parts of bilinguals to the Hebrew commentary, in fact still makes it hard to get a clear picture of the linguistics of the necropolis, but surely the main point is that the necropolis is quite simply chronologically irrelevant to this book. Sevenster has picked up a careless statement in *CIJ* to the effect that it dates from the first four centuries AD and adds his own unsupported belief that it is not absolutely impossible that some of these funerary inscriptions date from the first century, but ends by saying that 'even if the earliest date from the end of the second century, they form an important indication of the linguistic relationships in Palestine in a period close to the century with which we are occupied in particular'. Not all that close, I fear, and, considering the well-attested movement of population, I cannot believe that Beth Shearim has anything to tell us about the prevalence of Greek in pre-destruction Galilee.

Much further south, more substantial evidence is provided by the Murabba'at and Bar Cochba documents. As far as I can tell, Sevenster has been admirably pertinacious and successful in finding his way through the tangled bibliography which has preceded the final publication of the latter, and it is really here that we have the main counterpart in physical fact to the edict against teaching Greek of AD 118. Besides the three Semitic languages involved, Greek is very freely used, and again we must reasonably allow for a throwback of at least a generation or so behind these documents. Greek has penetrated even the most nationalist circles.

Sevenster's conclusions (pp. 176–91) are fairly predictable, that familiarity with Greek was possible and likely in all parts of the country and at all social levels in the first century AD. I would not like to go as || far as that. In Jerusalem and in the Hellenistic towns it is certain. Outside them, the direct evidence is distinctly sketchy, and the picture is likely to have been distinctly varied, depending on the amount of dealings one had with either the towns or the government. I would agree with Sevenster that there was a good deal of basic Greek about. There are, however, several stages beyond that. The ability to compose funerary inscriptions is a very elementary stage,

as a glance at the average Palestinian Greek funerary inscription will show. The ability to compose private letters is still only demonstrable for the second century. The ability to write literary Greek is a stage which Sevenster only jumps with the aid of the passage of Josephus discussed above, and the feeling that James had as special reasons for going to a good deal of trouble to write good Greek as Josephus, who certainly managed it. It does seem to me that there is a difference in the evidence about Galilee and Jerusalem, quite apart from questions about social class. Sevenster is clearly right to end by saying that 'the possibility can no longer be precluded that a Palestinian Jewish Christian of the first century AD wrote an epistle in good Greek', but I still think that James would have had to be an exceptional man to do it. Exceptional men can do anything, but this we knew already. Nevertheless, this seems to me a very useful book.

BIBLIOGRAPHY

This bibliography gives the publication details of books cited in the course of this volume, other than standard editions of texts (including epigraphic texts), excavation reports, such standard works as *CAH*, and a few works which are cited only once and for which details are given where they are cited.

ALTHEIM, F., and STIEHL, R. *Die aramäische Sprache unter den Achaimeniden*, 3 vols. Frankfurt a. M.: Klostermann, 1959–63.

AMANDRY, M. *Le Monnayage des duovirs corinthiens. BCH* Supp. 15 (1988).

ANDREADES, A. M., trans. Brown, C. N. *History of Greek Public Finance*, vol. i (all published). Harvard University Press, 1933.

Athenian Tribute Lists, The. See under Meritt, B. D., *et al.*

AUSTIN, R. P. *The Stoichedon Style in Greek Inscriptions*. Oxford University Press, 1938.

BAITER, J. G., and SAUPPE, H. (eds.), *Oratores Attici*, vol. ii. Zurich: Hoehr, 1850.

BECHTEL, F. *Die historische Personnamen des Griechischen bis zur Kaiserzeit*. Halle: Niemeyer, 1917.

BELOCH, K. J. *Griechische Geschichte*, 2nd edn, 4 vols. in 8. Strassburg: Trübner → Berlin and Leipzig: de Gruyter, 1912–27.

Beth She'arim, ii. *The Greek Inscriptions*. Jerusalem: Israel Exploration Society, 1967.

BOECKH, A. *Encyclopädie und Methodologie der philologischen Wissenschaften*, ed. BRATUSCHEK, E. Leipzig: Teubner, 1877. 2nd edn rev. KLUSSMANN, R., 1886.

Die Staatshaushaltung der Athener, 2 vols. Berlin: Realschulbuchhandlung, 1817. 3rd edn rev. FRÄNKEL, M. Berlin: Reimer, 1886. Trans. Lewis, G. C., *The Public Economy of Athens*. London: Parker, ²1842.

Urkunden über das Seewesen des attischen Staates, 2 vols. Berlin: Reimer, 1840.

BOEGEHOLD, A. L., and SCAFURO, A. C. (eds.), *Athenian Identity and Civic Ideology*. Johns Hopkins University Press, 1994.

BOEHRINGER, C. *Zur Chronologie mittelhellenistischer Münzserien, 220–160 v. Chr.*, 2 vols. Berlin: De Gruyter for Deutsches Archäologisches Institut, 1972.

BOND, G. W. *Euripides: Hypsipyle*. Oxford University Press, 1963.

BRADEEN, D. W., and MCGREGOR, M. F. *Studies in Fifth-Century Attic Epigraphy*. (University of Cincinnati Classical Studies, iv.) University of Oklahoma Press, 1973.

BRITISH MUSEUM. *Thracian Treasures from Bulgaria*. London: British Museum Publications, 1976.

BROMMER, F. *Festschrift für F. Brommer*. Mainz: Zabern, 1977.

BROUGHTON, T. R. S. *The Magistrates of the Roman Republic*, 2 vols. and a Supplement. New York: Am. Phil. Ass., 1951–60. (New Supplement, Atlanta: Scholars Press for Am. Phil. Ass., 1986.)

BUCHANAN, J. J. *Theorika*. Locust Valley: Augustin, 1962.

BURN, A. R. *The Lyric Age of Greece*. London: Arnold, 1960; revised 1978.

BUSOLT, G. *Griechische Geschichte bis zur Schlacht bei Chaeroneia*, vols. i², ii², iii. 1, iii. 2. Gotha: Perthes, 1893–1904.

BUSOLT, G., rev. SWOBODA, H. *Griechische Staatskunde*, 2 vols. (H.d.A. iv.1.1.) Munich: Beck, 1920–6.

CAMERON, G. G. *Persepolis Treasury Texts*. (Oriental Institute Publications, lxv.) University of Chicago Press, 1948.

CANFORA, L. *Inventario dei manoscritti greci di Demostene*. Padua: Antenore, 1968.

CARCOPINO, J. *L'Ostracisme athénien*, 2nd edn Paris: Alcan, 1935.

CAREY, C., and REID, R. A. *Demosthenes: Selected Private Speeches*. Cambridge University Press, 1985.

CARRADICE, I. (ed.), *Coinage and Administration in the Athenian and Persian Empires*. BAR International Series 343 (1987).

CARTLEDGE, P. *et al.* (eds.), *Nomos: Essays in Athenian Law, Politics and Society*. Cambridge University Press, 1990.

CAVAIGNAC, E. *Études sur l'histoire financière d'Athènes au V^e siècle*. Paris: Fontemoing, 1908.

CHAMOUX, F. *Cyrène sous la monarchie des Battiades*. (BEFAR clxxvii.) Paris: De Boccard, 1952.

CLARK, M. G. 'The Economy of the Athenian Navy in the Fourth Century BC', D. Phil. Diss. Oxford, 1993.

CONGRESS, ECONOMIC HISTORY. *Second International Conference of Economic History, 1962*, vol. i. *Trade and Politics in the Ancient World*. Paris and The Hague: Mouton, 1965.

EPIGRAPHIC. *3rd = Atti del terzo congresso internazionale di epigrafia greca e latina, 1957*. Rome: L'Erma di Bretschneider, 1959.

5th = Acta of the Fifth International Congress of Greek and Latin Epigraphy, 1967. Oxford: Blackwell, 1971.

8th = Πρακτικά τοῦ Η' Διεθνοῦς Συνεδρίου Ἑλληνικῆς καὶ Λατινικῆς Ἐπιγραφικῆς, *1982*, 2 vols. *Athens*: Ὑπουργεῖο Πολιτισμοῦ καὶ Ἐπιστημῶν, 1984–7.

NUMISMATIC. *Congresso Internazionale di Numismatica, 1961*, vol. ii. *Atti*. Rome: Ist. Ital. Num., 1965.

COOK, J. M. *The Persian Empire*. London: Dent, 1983.

COUAT, A. H. *Aristophane et l'ancienne comédie attique*. Paris: Lecène and Oudin, 1889; 3rd edn. Société Française d'Imprimerie et de Librairie, 1902.

COWLEY, A. E. *Aramaic Papyri of the Fifth Century B.C.* Oxford University Press, 1923.

CROISET, M. *Aristophane et les partis à Athènes*. Paris: Fontemoing, 1906. Trans. Loeb, J. *Aristophanes and the Political Parties at Athens*. London: Macmillan, 1909.

DANDAMAEV, M. A., trans. Vogelsang, W. J. *A Political History of the Achaemenid Empire*. (Original Russian edition 1985); Leiden: Brill, 1989.

DAUX, G. *Chronologie delphique*. Special fascicle of *Fouilles de Delphes*, iii. Paris: De Boccard for École Française d'Athènes, 1943.

Delphes au IIe at au Ier siècle. (BEFAR cxl.) Paris: De Boccard, 1936.

DAVIES, J. K. *Athenian Propertied Families, 600–300 B.C.* Oxford University Press, 1971.

DAY, J. *An Economic History of Athens under Roman Domination*. Columbia University Press, 1942.

DE FRANCISCIS, A. *Stato e società in Locri Epizefiri*. Naples: Libreria Scientifica Editrice for Univ. di Napoli, Cent. Stud. Magna Grecia, 1972.

DEGRASSI, A. *I fasti consolari dell'impero romano dal 30 a.C. al 613 d.C.* Rome: Ed. di Storia e Letteratura, 1952.

DE STE CROIX, G. E. M. *The Origins of the Peloponnesian War*. London: Duckworth, 1972.

DEUBNER, L. *Attische Feste*. Berlin: Keller, 1932. 2nd edn rev. DOER, B., Hildesheim: Olms, 1966.

DEVELIN, R. *Athenian Officials, 684–321 B.C.* Cambridge University Press, 1989.

DOVER, K. J. *Aristophanes: Clouds*. Oxford University Press, 1968.

Greek and the Greeks. (*Collected Papers*, i.) Oxford: Blackwell, 1987.

The Greeks and their Legacy. (*Collected Papers*, ii.) Oxford: Blackwell, 1988.

Lysias and the Corpus Lysiacum. (Sather Lectures, xxxix.) University of California Press, 1968.

DOW, S. *Prytaneis. Hesperia* Supp. 1 (1937).

Studies Presented to S. Dow. G. R. and B. Mon. 10 (1984).

DRERUP, E. *Demosthenes im Urteile des Altertums*. Würzburg: Becker, 1923.

DRIOTON, E., and VANDIER, J. *L'Égypte*, 3rd edn Paris: Presses Universitaires de France, 1952. (4th edn, 1962.)

DRIVER, G. R. *Aramaic Documents of the Fifth Century B.C.* Oxford University Press, 1954. Abridged and revised edn, 1957.

EDER, W. (ed.), *Die athenische Demokratie im 4. Jahrhundert v. Chr*. Stuttgart: Steiner, 1995.

EHRENBERG, V. *Neugründer des Staates*. Munich: Beck, 1925.

Ancient Society and Institutions: Studies Presented to V. Ehrenberg. Oxford: Blackwell, 1966.

ELIOT, C. W. J. *Coastal Demes of Attika. Phoenix* Supp. 5 (1962).

FARAGUNA, M. *Atene nell' età di Alessandro: problemi politici, economici, finanziari.* Rome: *Mem. Acc. Naz. Linc.* 9th ser. 2.ii, 1992.

FEHLING, D. *Die Quellenangaben bei Herodot.* Berlin: De Gruyter, 1971. Trans. Howie, J. G. *Herodotus and his 'Sources'.* Leeds: Cairns, 1989.

FERGUSON, W. S. *Hellenistic Athens.* London: Macmillan, 1911.

The Treasurers of Athena. Harvard University Press, 1932.

FIGUEIRA, T. J. *Athens and Aigina in the Age of Imperial Colonization.* Johns Hopkins University Press, 1991.

FINLEY, M. I. *The Ancient Economy.* (Sather Lectures, xliii.) University of California Press and London: Chatto and Windus, 1973.

(ed.), *Problèmes de la terre en Grèce ancienne.* Paris and The Hague: Mouton, 1973.

The Use and Abuse of History. London: Chatto and Windus, 1975.

FOCKE, F. *Demosthenesstudien.* Stuttgart: Kohlhammer, 1929.

FRANCOTTE, H. *Les Finances des cités grecques.* Paris: Champion, 1909.

L'Industrie dans la Grèce ancienne, 2 vols. Brussels: Société Belge de Librairie, 1900–1.

FREY, J. B. *Corpus Inscriptionum Judaicorum.* Rome: Pontifico Istituto di Archeologia Cristiana, 1936.

FRYE, R. N. *The History of Ancient Iran.* (H.d.A. iii.7.) Munich: Beck, 1984.

GAEBLER, H. *Die antiken Münzen von Makedonia und Paionia,* 2 vols. (Die antiken Münzen Nord-Griechenlands, iii.) Berlin: Reimer → de Gruyter, 1906–35.

GARRISON, M. B., and ROOT, M. C. *Persepolis Seal Studies: An Introduction with Provisional Concordances on Fortification Tablets 1–2087.* Leiden: Brill, 1995.

GAUTHIER, P. *Un Commentaire historique des Poroi de Xénophon.* Geneva: Droz and Paris: Minard for École Pratique des Hautes Études, Centre de Recherches d'Histoire et de Philologie, 1976.

GERNET, L. *Anthropologie de la Grèce antique.* Paris: Maspero, 1968.

GIBSON, M., and BIGGS, R. D. (eds.), *Seals and Sealing in the Ancient Near East.* Malibu: Undena, 1977.

GLOTZ, G., with COHEN, R. *Histoire générale: histoire grecque,* 4 vols. Paris: Presses Universitaires de France, 1925–39.

Mélanges G. Glotz, 2 vols. Paris: Presses Universitaires de France, 1932.

GOMME, A. W. *More Essays in Greek History and Literature.* Oxford: Blackwell, 1962.

ANDREWES, A., and DOVER, K. J. *A Historical Commentary on Thucydides,* 5 vols. Oxford University Press, 1945–81.

GOODWIN, W. W. *Demosthenes Against Midias,* Cambridge University Press, 1906.

GRAHAM, A. J. *Colony and Mother City in Ancient Greece.* Manchester University Press, 1964.

GRAINDOR, P. *Athènes de Tibère à Trajan.* Cairo: Misr for Université Égyptienne, 1931.

Athènes sous Hadrien. Cairo: Ministry of Public Instruction, 1931.

Hérode Atticus. Cairo: Misr for Université Égyptienne, 1930.

GRAYSON, A. K. *Assyrian Royal Inscriptions*, ii. Wiesbaden: Harrassowitz, 1976.

GRAYSON, C. H. 'Weighing in Ancient Greece', D. Phil. Diss. Oxford, 1975.

GREEN, P. *Alexander of Macedon*. Harmondsworth: Penguin, 1974.

GREENFIELD, J. C., and PORTEN, P. *The Bisitun Inscription of Darius the Great: Aramaic Version*. (Corpus Inscriptionum Iranicarum, i.5.1.) London: Lund Humphries, 1982.

GROAG, E. *Die Reichsbeamten von Achaia in spätrömischen Zeit*. Budapest: Institut für Münzkunde und Archaeologie der P. Pázmány-Universität, 1946.

GROTE, G. *A History of Greece*, 'new edition'. 12 vols.: London: Murray, 1869 (repr. 1884). 10 vols.: 1888.

GRÜNER, W. *Korinths Verfassung und Geschichte, mit besonderer Berücksichtigung seines Politik, während der Pentekontaetie*. (Diss. Leipzig.) Colditz: Heinke, n.d. (*c.* 1875).

GRUNDY, G. B. *Thuydides and the History of his Age*. London: Murray, 1911. Vols. i², ii, Oxford: Blackwell, 1948.

HABICHT, C. *Untersuchungen zur politischen Geschichte Athens im 3. Jahrhundert v. Chr.* (Vestigia, xxx.) Munich: Beck, 1979.

HALLOCK, R. T. *Persepolis Fortification Tablets*. (Oriental Institute Publications, xcii.) University of Chicago Press, 1969.

HAMMOND, N. G. L. *A History of Greece to 322 B.C.* Oxford University Press, 1959.

GRIFFITH, G. T., and WALBANK, F. W. *A History of Macedonia*, 3 vols. Oxford University Press, 1972–88.

HANSEN, P. A. *Carmina Epigraphica Graeca*, 2 vols. (Texte und Commentare, xii, xv). Berlin: De Gruyter, 1983–9.

HARRISON, A. R. W. *The Law of Athens*, 2 vols. Oxford University Press, 1968–71.

HATZFELD, J. *Alcibiade*. Paris: Presses Universitaires de France, 1940; 2nd edn, 1951.

HAUSSOULLIER, B. *Études sur l'histoire de Milet et du Didymeion*. Paris: Bouillon, 1902.

HEAD, B. V. *Historia Numorum*, 2nd edn. Oxford University Press, 1911.

HERINGTON, C. J. *Athena Parthenos and Athena Polias*. Manchester University Press, 1955.

HIGNETT, C. *A History of the Athenian Constitution to the End of the Fifth Century B.C.* Oxford University Press, 1952.

HINZ, W. *Darius und die Perser: Eine Kulturgeschichte der Achämeniden*, 2 vols. Baden-Baden: Holle, 1976–9.

Neue Wege im Altpersischen. (Göttinger Orientforschungen, iii.1.) Wiesbaden: Harrassowitz, 1973.

JACOB, O. *Les Esclaves publics à Athènes*. Liège: Vaillant-Carmanne and Paris: Champion, 1928.

JACOBY, F. *Abhandlungen zur griechischen Geschichtschreibung*. Leiden: Brill, 1956.

JAEGER, W. *Demosthenes*. (Sather Lectures, xiii.) University of California Press / Cambridge University Press, 1938.

JEFFERY, L. H. *Archaic Greece: The City-States, c. 700–500 B.C.* London and Tonbridge: Benn, 1976.

The Local Scripts of Archaic Greece. Oxford University Press, 1961. Revised with a Supplement by JOHNSTON, A. W., 1990.

JONES, A. H. M. *Athenian Democracy*. Oxford: Blackwell, 1957.

JUDEICH, W. *Topographie von Athen*. (H.d.A. III.2.ii.) 2nd edn, Munich: Beck, 1931.

KENNEDY, G. A. *Aristotle on Rhetoric: A Theory of Civic Discourse*. Oxford University Press, 1991.

KIENITZ, F. K. *Die politische geschichte Ägyptens vom 7. bis zum 4. Jahrhundert vor der Zeitwende*. Berlin: Akademie-Verlag, 1953.

KNAUTH, W., and NADJMABADI, S. *Das altiranische Fürstenideal von Xenophon bis Ferdausi*. Wiesbaden: Steiner, 1975.

KOCH, H. *Persien zur Zeit des Dareios: Das Achämenidenreich im Lichte neuer Quellen*. Marburg: Philipps-Universität, 1988.

Die religiösen Verhältnisse des Dareioszeit: Untersuchungen an Hand der elamischen Persepolistäfelchen. Wiesbaden: Harrassowitz, 1977.

KRAELING, E. G. *The Brooklyn Aramaic Papyri*. Yale University Press for Brooklyn Museum, 1953.

KRENTZ, P. *The Thirty at Athens*. Cornell University Press, 1982.

LANGLOTZ, E. *Charites: Studien zur Altertumswissenschaft* [*E. Langlotz gewidmet*]. Bonn: Athenäum, 1957.

LARSEN, J. A. O. *Representative Government in Greek and Roman History*. (Sather Lectures, xxviii.) University of California Press, 1955.

LATTIMORE, R. (trans.), *The Odes of Pindar*. University of Chicago Press, 1947; 2nd edn, 1976.

LAUFFER, S. *Die Bergwerkssklaven von Laureion*. (*Abh. Mainz* 1955, xii.) Wiesbaden: Steiner, 1955; 2nd edn, 1979.

LEHNER, H. *Ueber die athenischen Schatzverzeichnisse des vierten Jahrhunderts*. Diss. Strassburg, 1890.

LERAT, L. *Les Locriens de l'Ouest*. (BEFAR clxxvi.) Paris: De Boccard, 1952.

LÉVÊQUE, P. *Pyrrhos*. (BEFAR cxlv.) Paris: De Boccard, 1957.

LEWIS, D. M. *Sparta and Persia*. (Cincinnati Classical Studies, n.s. i.) Leiden: Brill, 1977.

'Towards a Historian's Text of Thucydides', Diss. Princeton, 1952.

LIPSIUS, J. H. *Das attische Recht und Rechtsverfahren*. 3 vols. in 4. Leipzig: Reisland, 1905–15.

LOSSAU, M. J. *Untersuchungen zur antiken Demosthenesexegese*. (Diss. Saarbrücken, 1962.) (Palingenesia, ii.) Bad Homburg: Gehlen, 1964.

MACDOWELL, D. M. *Demosthenes Against Meidias*. Oxford University Press, 1990.

MCGREGOR, M. F. *The Athenians and their Empire*. University of British Columbia Press, 1987.

Classical Contributions Presented to M. F. McGregor. Locust Valley NY: Augustin, 1981.

MALLET, D. *Les Rapports des Grecs avec l'Égypte (de la conquête de Cambyse, 525, à celle d'Alexandre, 331)*. Cairo: Inst. Fr. d'Arch. Or., 1922.

MARCADÉ, J. *Recueil des signatures de sculpteurs grecs*, 2 vols. Paris: De Boccard, 1953–8.

MARSHALL, F. H. *The Second Athenian Confederacy*. Cambridge University Press, 1905.

MARTIN, T. R. *Sovereignty and Coinage in Classical Greece*. Princeton University Press, 1985.

MAYRHOFER, M. *Onomastica Persepolitana: Das altiranische Namengut der Persepolis-Täfelchen. Sb. Wien* 286 (1973).

MAZUR, B. D. *Studies on Jewry in Greece*, vol. i (all published). Athens: Hestia, 1935.

MEIGGS, R. *The Athenian Empire*. Oxford University Press, 1972.

and LEWIS, D. M. *A Selection of Greek Historical Inscriptions to the End of the Fifth Century B.C.* Oxford University Press, 1969; revised, 1988.

MEISTERHANS, K., rev. SCHWYZER, E. *Grammatik der attischen Inschriften*, 3rd edn. Berlin: Weidmann, 1900.

MENDEL, G. *Musées Impériaux Ottomans: Catalogue des sculptures grecques, romaines et byzantines*, 3 vols. Constantinople: Musées Impériaux Ottomans, 1912–14.

MERITT, B. D. *Athenian Financial Documents of the Fifth Century*. (Humanistic Series, xxvii.) University of Michigan Press, 1932.

The Athenian Year (Sather Lectures, xxxiii.) University of California Press, 1961.

and WEST, A. B. *The Athenian Assessment of 425 B.C.* (Humanistic Series, xxxiii.) University of Michigan Press, 1934.

et al. The Athenian Tribute Lists, 4 vols. Harvard University Press for Am. Sch. Class. Stud. Ath. → Princeton: Am. Sch. Class. Stud. Ath., 1939–53.

Φόρος: *Tribute to B. D. Meritt*. Locust Valley, NY: Augustin, 1974.

MEYER, E. *Forschungen zur alten Geschichte*, 2 vols. Halle: Niemeyer, 1892–8.

Geschichte des Altertums. Final edn rev. STIER, E., 5 vols. in 9. Stuttgart: Cotta → Basle and Stuttgart: Schwabe, 1925–58.

MICHAELIS, A. *Der Parthenon*. Leipzig: Breitkopf and Härtel, 1871.

MILCHHOEFER, A. *Untersuchungen über die Demenordnung des Kleisthenes. Abh. Berlin* (1892).

MISSAILIDOU-DESPOTIDOU, V. 'A Thessalian Inscription and its Topographical Implications'. M. Phil. Diss. Oxford, 1986.

MOMIGLIANO, A. *Alien Wisdom: The Limits of Hellenization*. Cambridge University Press, 1975.

Contributo alla storia degli studi classici e del mondo antico. Rome: Ed. di Storia e Letteratura, 1955.

Sesto contributo alla storia degli studi classici e del mondo antico, 2 vols. Rome: Ed. di Storia e Letteratura, 1980.

MOMMSEN, T. *Festschrift T. Mommsen*. Marburg: Elwert, 1893.

MOTZKI, A. *Eubulos von Probalinthos und seine Finanzpolitik*. (Diss.) Königsberg in Preussen: Leupold, 1903.

MURRAY, G. *Aristophanes: A Study*. Oxford University Press, 1933.

Greek Poetry and Life: Essays Presented to G. Murray. Oxford University Press, 1936.

MURRAY, O., and PRICE, S. *The Greek City from Homer to Alexander*. Oxford University Press, 1990.

MYLONAS, G. E. *Aghios Kosmas*. Princeton University Press, 1959.

NILSSON, M. P. *Cults, Myths, Oracles and Politics in Ancient Greece*. Lund: Gleerup, 1951.

Griechische Feste von religiöser Bedeutung, mit Ausschluß der attischen. Leipzig: Teubner, 1906.

Primitive Time Reckoning. Lund: Gleerup, 1920.

OHLY, K. *Stichometrische Untersuchuungen*. (*ZBB* Beiheft lxi.) Leipzig: Harrassowitz, 1928.

OLMSTEAD, A. T. *History of the Persian Empire (Achaemenid Period)*. University of Chicago Press, 1948.

Or des scythes: trésors des musées soviétiques. Paris: Secrétariat d'État à la Culture, 1975.

OSBORNE, M. J. *Naturalization in Athens*, 4 vols. in 3. Brussels: *Verh. Kon. Ac. Wet., Klasse Let.* xcviii, ci, cix, 1981–3.

OSBORNE, R. G. *Demos: The Discovery of Classical Attica*. Cambridge University Press, 1985.

OSTWALD, M. *From Popular Sovereignty to the Sovereignty of Law*. University of California Press, 1986.

PAGE, D. L. *Dionysiaca . . . Sir D. Page*, ed. DAWE, R. D. *et al.* Cambridge: the editors, 1978.

PARKE, H. W. *Festivals of the Athenians*. London: Thames and Hudson, 1977.

PARKER, R. H., and DUBBERSTEIN, W. H. *Babylonian Chronology, 626 B.C.–A.D. 45*, 2nd edn, University of Chicago Press, 1946. 3rd edn. . . . *626 B.C.–A.D. 75*, Brown University Press, 1956.

PERLMAN, S. (ed.), *Philip and Athens*. Cambridge: Heffer, 1973.

PFUHL, E. *Die Anfänge der griechsichen Bildniskunst*. Munich: Bruckmann, 1927.

PHILIPPSON, A., ed. LEHMANN, H., and KIRSTEN, E. *Die griechischen Landschaften*, 4 vols. in 8. Frankfurt a. M.: Klostermann, 1950–9.

PICARD, C. *Manuel d'archéologie grecque: La Sculpture*, 5 vols. Paris: Picard, 1935–66.

PICKARD-CAMBRIDGE, A. W. *Dithyramb, Tragedy and Comedy*. Oxford University Press, 1927; revised by WEBSTER, T. B. L., 1962.

PIÉRART, M. (ed.), *Aristote et Athènes: Aristoteles and Athens*. Paris: De Boccard for Université de Fribourg, Séminaire d'Histoire Ancienne, 1993.

PLATNAUER, M. *Fifty Years of Classical Scholarship*. Oxford: Blackwell, 1954. 2nd edn, *Fifty Years (and Twelve) of Classical Scholarship*, 1968.

PORTEN, B. *Archives from Elephantine: The Life of an Ancient Jewish Military Colony*. University of California Press, 1968.

POUILLOUX, J. *La Forteresse de Rhamnonte*. (BEFAR clxxix.) Paris: De Boccard, 1954.

PRIDIK, A. *De Cei insulae rebus*. Diss. Berlin, 1892.

PRITCHARD, J. B. *Ancient Near Eastern Texts Relating to the Old Testament*, 3rd edn. Princeton University Press, 1969.

PRITCHETT, W. K., and MERITT, B. D. *The Chronology of Hellenistic Athens*. Harvard University Press for Am. Sch. Class. Stud. Ath., 1940.

QUASS, F. *Nomos und Psephisma: Untersuchung zur griechischen Staatsrecht*. (Zetemata, lv.) Munich: Beck, 1971.

RANKE, H. *Die aegyptischen Personnamen*, 2 vols. Glückstadt: Augustin, 1935–52.

RAUBITSCHEK, A. E., with JEFFERY, L. H. *Dedications from the Athenian Akropolis*. Cambridge, Mass.: Archaeological Institute of America, 1949.

The Greek Historians: Literature and History: Papers Presented to A. E. Raubitschek. Saratoga, Calif.: ANMA Libri for Dept of Classics, Stanford University, 1985.

Recherches sur l'historie et les cultes de Thasos, ed. DUNANT, C. (vol. ii) and POUILLOUX, J. 2 vols. (Études Thasiennes iii/v.) Paris: De Boccard for École Française d'Athènes, 1954/7.

RHODES, P. J. *The Athenian Boule*. Oxford University Press, 1972.

A Commentary on the Aristotelian Athenaion Politeia. Oxford University Press, 1981.

with LEWIS, D. M. *The Decrees of the Greek States*. Oxford University Press, 1997.

RICHTER, G. M. A. *The Archaic Gravestones of Attica*. London: Phaidon, 1961.

and MILNE, M. J. *Shapes and Names of Athenian Vases*. New York: Metropolitan Museum of Art, 1935.

ROBERT, L. *Études de numismatique grecque*. Paris: Collège de France, 1951.

Études épigraphiques et philologiques. Paris: Champion for École des Hautes Études, 1938.

Hellenica, 13 vols. in 12. Limoges: Bontemps → Paris: Maisonneuve, 1940–65.

ROBINSON, D. M. *Studies Presented to D. M. Robinson*, 2 vols. St Louis, Missouri: Washington University, 1951–3.

ROBINSON, E. S. G. *Essays in Greek Coinage . . . S. Robinson*. Oxford University Press, 1968.

ROUX, G. *L'Amphictionie, Delphes et le temple d'Apollon*. Paris: De Boccard for Lyon: Maison de l'Orient, 1979.

RYDER, T. T. B. *Koine Eirene*. Oxford University Press for University of Hull, 1965.

SALMON, J. B. *Wealthy Corinth*. Oxford University Press, 1984.

SANCISI-WEERDENBURG, H. W. A. M. *Yaunā en Persai: Grieken en Perzen in een ander Perspectief.* (Diss. Leiden.) Groningen: Niemeyer, 1980.

SANDYS, J. E. *History of Classical Scholarship*, vols i³/ii–iii. Cambridge University Press, 1921/1908.

SANFILIPPO, C. *Studi in onore di C. Sanfilippo*, 7 vols. Milan: Giuffrè for Fac. Giurisprud. Univ. Catania, 1982–7.

SCHACHERMEYR, F. *Greece and the Eastern Mediterranean in Ancient History and Prehistory: Studies Presented to F. Schachermeyr.* Berlin and New York: De Gruyter, 1977.

SCHACHTER, A. *Essays in the Topography, History and Culture of Boiotia.* Montreal: *Teiresias* Supp. iii (1990).

SCHAEFER, A. *Demosthenes und seine Zeit*, 3 vols., 2nd edn. Leipzig: Teubner, 1885–7.

SCHMID, W. *Geschichte der griechischen Literatur: Die klassische Periode*, vol. iv.2.1. (H.d.A. vii.1.4.2.1.) Munich: Beck, 1946.

SCHMIDT, E. F. *Persepolis*, iii. *The Royal Tombs and Other Monuments.* (Oriental Institute Publications, lxx.) University of Chicago Press, 1970.

SCHULLER, W. *Die Herrschaft der Athener im Ersten Attischen Seebund.* Berlin: De Gruyter, 1974.

SCHWENK, C. J. *Athens in the Age of Alexander.* Chicago: Ares, 1985.

SEVENSTER, J. R. *Do You Know Greek?* (*Novum Testamentum* Supp. xix.) Leiden: Brill, 1968.

SIEWERT, P. *Die Trittyen Attikas und die Heeresreform des Kleisthenes.* (Vestigia, xxxiii.) Munich: Beck, 1982.

SIMON, E. *Festivals of Attica: An Archaeological Commentary.* University of Wisconsin Press, 1983.

SOLDERS, S. *Die ausserstädtischen Kulte und die Einigung Attikas.* Lund: Lindstedt, 1931.

STARR, C. G. *Athenian Coinage, 480–449 B.C.* Oxford University Press, 1970.

STEVENS, C. E. *The Ancient Historian and his Materials . . . C. E. Stevens.* Farnborough: Gregg International, 1975.

STRONG, D. E. *Greek and Roman Gold and Silver Plate.* London: Methuen, 1966.

STROUD, R. S. *Drakon's Law on Homicide.* (U. Calif. Pub. Class. Stud., iii.) University of California Press, 1968.

SZANTO, E. *Die griechischen Phylen. Sb. Wien* 144.5 (1902).

THOMASSON, B. E. *Laterculi Praesidum*, 3 vols. in 5. Goteborg: Radius, 1984–90.

THOMPSON, M. *The New Style Silver Coinage of Athens*, 2 vols. New York: American Numismatic Society, 1961.

THOMSEN, R. *The Origin of Ostracism.* Copenhagen: Gyldendal, 1972.

THREATTE, L. *Grammar of Attic Inscriptions.* Berlin: de Gruyter, 1980– .

TOEPFFER, J. *Attische Genealogie.* Berlin: Weidmann, 1889.

TOMLINSON, R. A. *Argos and the Argolid.* London: Routledge, 1972.

TRAILL, J. S. *The Political Organization of Attica. Hesperia* Supp. 14 (1975).

TRAVLOS, J. *Pictorial Dictionary of Ancient Athens*. London: Thames and Hudson for Deutsches Archäologisches Institut, 1971.

TRÉHEUX, J. *Comptes et inventaires dans la cité grecque . . . en l'honneur de J. Tréheux*. Geneva: Droz for Fac. Lett. Univ. Neuchâtel, 1988.

UGUZZONI, A., and GHINATTI, F. *Le tavole greche di Eraclea*. (U. di Padova Pubb. Ist. Stor. Ant. vii.) Rome: L'Erma di Bretschneider, 1968.

URE, P. N. *The Origin of Tyranny*. Cambridge University Press, 1922.

VENEDIKOV, I., and GERASSIMOV, T. *Thracian Art Treasures*. Sofia: Bulgarski Houdozhnik, 1975.

VICKERS, M. (ed.), *Pots and Pans: A Colloquium on Precious Metals and Ceramics in the Muslim, Chinese and Graeco-Roman Worlds, 1985*. (Oxford Studies in Islamic Art iii.) Oxford University Press for University of Oxford, Faculty of Oriental Studies, 1986.

VOIGTLANDER, E. N. VON. *The Bisitun Inscription of Darius the Great: Babylonian Version*. (Corpus Inscriptionum Iranicarum, i.2.1.) London: Lund Humphries, 1978.

WADE-GERY, H. T. *Essays in Greek History*. Oxford: Blackwell, 1958.

WALBANK, M. B. *Athenian Proxenies of the Fifth Century B.C.* Toronto and Sarasota, Fla.: Stevens, 1978.

WALLACE, W. P. *The Euboean League and its Coinage*. (Numismatic Notes and Monographs cxxxiv.) New York: American Numismatic Society, 1956.

WEHRLI, F. *Hermippos der Kallimacheer*. (*Die Schule des Aristoteles*, Supp. i.) Basel: Schwabe, 1974.

WEST, M. L. *Hesiod: Theogony*. Oxford University Press, 1966.

WHITEHEAD, D. *The Demes of Attica, 508/7 – ca. 250 B.C.* Princeton University Press, 1986.

WILAMOWITZ-MOELLENDORFF, U. VON. *Aristoteles und Athen*, 2 vols. Berlin: Weidmann, 1893.

Athenion und Aristion. Sb. Berlin 1923, no. 7 = *Kleine Schriften*, v. 204–19.

Geschichte der Philologie. (GERCKE, A., and NORDEN, E. (eds.), Einleitung in die Altertumswissenschaft, i.1.) Leipzig: Teubner, 3rd edn 1927. Trans. Harris, A. *History of Classical Scholarship*. London: Duckworth, 1982.

Kleine Schriften, 6 vols. in 7. Berlin: Weidmann → Akademie-Verlag, 1935–72.

Pindaros. Berlin: Weidmann, 1922.

Reden und Vorträge, 3rd edn. Berlin: Weidmann, 1913.

and ROBERT, C. *Aus Kydathen*. Berlin: Weidmann, 1880.

WILHELM, A. *Abhandlungen und Beiträge zur griechischen Inschriftenkunde*, 2 vols. (Opuscula, viii.2.) Leipzig: Zentralantiquariat der D. D. R., 1984.

Akademieschriften zur griechischen Inschriftenkunde, 3 vols. (Opuscula, viii.1.) Leipzig: Zentralantiquariat der D. D. R., 1974.

WILL, E. *Korinthiaka*. Paris: De Boccard, 1955.

et al. Le Monde grec et l'Orient, 2 vols. Paris: Presses Universitaires de France, 1972–5.

PUBLICATIONS OF DAVID M. LEWIS

(Where the nominal year and the actual year of publication are adjacent years, the nominal year only is given. An asterisk indicates that the work in question appears in this volume.)

1952

'Towards a Historian's Text of Thucydides', Diss. Princeton.

1954

'Notes on Attic Inscriptions', *BSA* 49 (1954), 17–50.

'Ithome Again', *Historia* 2 (1953–4), 412–18.

1955

'Notes on Attic Inscriptions, II', *BSA* 50 (1955), 1–37.

* Includes 'Who was Lysistrata?', pp. 1–12 = this volume, ch. 22.

'The President's House', *Pelican Record* 31 (1954–7), 51–4.

'The Public Seal of Athens', *Phoenix* 9 (1955), 32–4.

1956

'The Deme Ikarion', *BSA* 51 (1956), 172.

1957

* 'The First Greek Jew', *Journal of Semitic Studies* 2 (1957), 264–6 = this volume, ch. 37.

(with A. Andrewes) 'Note on the Peace of Nikias', *JHS* 77 (1957), 177–80.

REVIEWS

T. B. L. Webster, *Art and Literature in Fourth Century Athens*. *JHS* 77 (1957), 328.

B. Hemmerdinger, *Essai sur l'histoire du texte de Thucydide*. *JHS* 77 (1957), 329–30.

1958

'When was Aeschines Born?' *CR* n.s. 8 (1958), 108.

'An Aristotle Publication-Date', *CR* n.s. 8 (1958), 108.

* 'The Phoenician Fleet in 411', *Historia* 7 (1958), 392–7 = this volume, ch. 35.

1959

'Attic Manumissions', *Hesperia* 28 (1959), 208–38.

* 'Law on the Lesser Panathenaia', *Hesperia* 28 (1959), 239–47 = this volume, ch. 27.

'Athens and Troizen', *Hesperia* 28 (1959), 248–50.

REVIEW

'The Testimony of Stones': review of A. G. Woodhead, *The Study of Greek Inscriptions. The Listener* (20 August 1959), 281, 284.

1960

* 'Apollo Delios', *BSA* 55 (1960), 190–4 = this volume, ch. 19.

'Olympiad', *Notes and Queries* n.s. 7 (1960), 403.

REVIEW

A. Lambrechts, *Tekst en uitzicht van de Atheense proxeniedecreten tot 323 v. C. Gnomon* 32 (1960), 166–7.

1961

'Notes on the Decree of Themistocles', *CQ* n.s. 11 (1961), 61–6.

REVIEW

C. Hude and O. Luschnat, *Thucydides: Historiae*, vol. i. *JHS* 81 (1961), 172.

1962

'The Archon of 497/6 BC', *CQ* n.s. 12 (1962), 201.

* 'The Federal Constitution of Keos', *BSA* 57 (1962), 1–4 = this volume, ch. 4.

* 'The Chronology of the Athenian New Style Coinage', *NC* 7th series 2 (1962), 275–300 = this volume, ch. 29.

REVIEW

* M. Thompson, *The New Style Silver Coinage of Athens. CR* n.s. 12 (1962), 290–2 = this volume, ch. 30.

1963

* 'Cleisthenes and Attica', *Historia* 12 (1963), 22–40 = this volume, ch. 10.

REVIEWS

D. M. MacDowell, *Andokides: On the Mysteries. The Oxford Magazine* (2 May 1963), 268.

C. W. J. Eliot, *Coastal Demes of Attika. Gnomon* 35 (1963), 723–5.

L. H. Jeffery, *The Local Scripts of Archaic Greece. JHS* 83 (1963), 175–6.

B. D. Meritt, *The Athenian Year. JHS* 83 (1963), 195–6.

G. E. Mylonas, *Eleusis and the Eleusinian Mysteries. JHS* 83 (1963), 206–7.

1964

Appendix I: 'The Jewish Inscriptions of Egypt' in V. A. Tcherikover, A. Fuks and M. Stern (eds.), *Corpus Papyrorum Judaicarum* iii.138–66.

'The Dedication of Aristokrates', *JHS* 84 (1964), 156–7.

REVIEW

A. R. Burn, *Persia and the Greeks. JHS* 84 (1964), 204–5.

1965

REVIEWS

A. French, *The Development of the Athenian Economy. Econ. Hist. Rev.* n.s. 18 (1965), 443–4.

P. Lévêque and P. Vidal-Naquet, *Clisthène l'Athénien. JHS* 85 (1965), 222–3.

1966

* 'After the Profanation of the Mysteries', *Ancient Society and Institutions . . . V. Ehrenberg* 177–91 = this volume, ch. 20.

REVIEWS

A. Kleinlogel, *Geschichte des Thukydidestextes im Mittelalter. Gnomon* 38 (1966), 135–8.

G. Ramming, *Die politischen Ziele und Wege des Aischines. CR* n.s. 16 (1966), 406.

1967

* 'A Note on *IG* i² 114', *JHS* 87 (1967), 132 = this volume, ch. 23.

REVIEW

G. Pfohl, *Griechische Inschriften als Zeugnisse des privaten und öffentlichen Lebens. CR* n.s. 17 (1967), 321–3.

1968

(with J. Gould) A. W. Pickard-Cambridge, *The Dramatic Festivals of Athens* 2nd edn. Oxford University Press (See also under 1988.)

'Delendum?' *CR* n.s. 18 (1968), 267.

'Dedications of Phialai at Athens', *Hesperia* 37 (1968), 368–80.

'New Evidence for the Gold-Silver Ratio', *Essays in Greek Coinage . . . Stanley Robinson*, 105–10.

REVIEWS

B. Lifschitz, *Donateurs et fondateurs dans les synagogues juives. JTS* n.s. 19 (1968), 644–5.

J. A. O. Larsen, *Greek Federal States. JHS* 88 (1968), 219–20.

S. Dow and R. F. Healey, *A Sacred Calendar of Eleusis. CR* n.s. 18 (1968), 357.

1969

(with R. Meiggs) *A Selection of Greek Historical Inscriptions to the End of the Fifth Century B.C.* Oxford University Press (See also under 1988 and 1989.)

'Two Days: (i) Epicurus's birthday, (ii) Alexander's Death-Day', *CR* n.s. 19 (1969), 271–2.

REVIEWS

G. W. Bowersock (ed.), *Xenophon: Scripta Minora. CR* n.s. 19 (1969), 45–7.

E. J. Bickerman, *Chronology of the Ancient World. CR* n.s. 19 (1969), 110–11.

D. J. Geagan, *The Athenian Constitution after Sulla. CR* n.s. 19 (1969), 111–12.

C. A. P. Ruck, *IG ii² 2323: The List of the Victors at the Dionysia. CR* n.s. 19 (1969), 243–4.

D. Diringer, *The Alphabet. CR* n.s. 19 (1969), 390.

* J. N. Sevenster, *Do You Know Greek? JTS* n.s. 20 (1969), 583–8 = this volume, ch. 38.

1970

'Aristophanes, *Clouds* 64', *CR* n.s. 20 (1970), 288–9.

* 'Preliminary Notes on the Locri Archive', *Klio* 52 (1970), 247–53 = this volume, ch. 6.

REVIEWS

A. Hönle, *Olympia in der Politik der griechischen Staatenwelt. CR* n.s. 20 (1970), 253–4.

C. Habicht, *Altertümer von Pergamon*, VIII.iii. *CR* n.s. 20 (1970), 406–8.

1971

* 'Boeckh, *Staatshaushaltung der Athener*, 1817–1967', *Acta of the Fifth International Congress of Greek and Latin Epigraphy, Cambridge, 1967*, 35–9 = this volume, ch. 1.

REVIEWS

S. Dow, *Conventions in Editing. CR* n.s. 21 (1971), 309–10.

R. S. Stroud, *Drakon's Law on Homicide. CR* n.s. 21 (1971), 390–1.

J. Crampa, *Labraunda: The Greek Inscriptions*, i. *CR* n.s. 21 (1971), 118–19.

(with A. Morpurgo Davies) A. Uguzzoni and F. Ghinatti, *Le tavole greche di Eraclea. CR* n.s. 21 (1971), 119–22.

M. J. Fontana, *L'Athenaion Politeia del V secolo a.C. CR* n.s. 21 (1971), 126.

H. H. Schmitt, *Die Staatsverträge des Altertums*, iii. *CR* n.s. 21 (1971), 296–7.

P. Herrmann and K. Z. Polatkan, *Das Testament des Epikrates. CR* n.s. 21 (1971), 466.

1972

REVIEWS

H. W. Pleket, *Epigraphica*, ii. *CR* n.s. 22 (1972), 130.

W. K. Pritchett, *The Choiseul Marble. CR* n.s. 22 (1972), 256–9.

S. M. Stern, *Aristotle on the World-State. CR* n.s. 22 (1972), 271.

1973

* 'The Athenian *Rationes Centesimarum*', M. I. Finley (ed.), *Problèmes de la terre en Grèce ancienne* 187–212.

'*G.V.I.* 734', *CR* n.s. 23 (1973), 9.

'Themistocles' Archonship', *Historia* 22 (1973), 757–8.

REVIEWS

G. Bodei Giglioni (ed.), *Xenophontis de Vectigalibus. CR* n.s. 23 (1973), 88.

H. Bengtson, *Introduction to Ancient History. CR* n.s. 23 (1973), 102–3.

L. Robert, *Die Epigraphik der klassischen Welt. CR* n.s. 23 (1973), 110–11.

O. W. Reinmuth, *The Ephebic Inscriptions of the Fourth Century B.C. CR* n.s. 23 (1973), 254–6.

N. Lewis, *Greek Historical Documents: The Fifth Century B.C. CR* n.s. 23 (1973), 283–4.

1974

'Pericles', *Encyclopaedia Britannica*, 15th edn, vol. xiv, 66–8.

* 'Entrenchment-Clauses in Attic Decrees', Φόρος . . . *B. D. Meritt* 81–9 = this volume, ch. 18.

'The Altar of the Eudanemoi', *CR* n.s. 24 (1974), 186–7.

* 'The Kerameikos Ostraka', *ZPE* 14 (1974), 1–4 = this volume, ch. 13.

REVIEWS

J. Travlos, *Pictorial Dictionary of Ancient Athens. CR* n.s. 24 (1974), 110–12.

Institut Fernand-Courby, *Nouveau Choix d'inscriptions grecques. CR* n.s. 24 (1974), 148.

F. Quass, *Nomos und Psephisma. CR* n.s. 24 (1974), 300–1.

E. Schürer, rev. and ed. G. Vermes and F. Millar, *The History of the Jewish People in the Age of Jesus Christ* i. *JTS* n.s. 25 (1974), 481–3.

1975

'Bibliography of A. M. Woodward', *BSA* 70 (1975), 177–82.

'Greek Inscriptions from the Athenian Agora', *Hesperia* 44 (1975), 379–95.

A. M. Woodward, edited by D. M. Lewis, 'A Transfer from Eleusis', *BSA* 70 (1975), 183–8.

REVIEWS

A. E. Samuel, *Greek and Roman Chronology. CR* n.s. 25 (1975), 69–72.

W. R. Connor, *The New Politicians of Fifth Century Athens. CR* n.s. 25 (1975), 87–90.

E. Posner, *Archives in the Ancient World. CR* n.s. 25 (1975), 154.

I. A. Bartsos, Ἀθηναϊκαὶ κληρούχιαι. *CR* n.s. 25 (1975), 155.

A. Giovannini, *Untersuchungen über die Natur und die Anfänge der bundesstaatlichen Sympolitie in Griechenland. CR* n.s. 25 (1975), 155.

W. Peek, *Epigramme und andere Inschriften aus Lakonien und Arkadien. CR* n.s. 25 (1975), 160.

P. Gauthier, *Symbola. CR* n.s. 25 (1975), 262–3.

T. Linders, *Studies in the Treasure Records of Artemis Brauronia Found in Athens. CR* n.s. 25 (1975), 326.

J. Crampa, *Labraunda: The Greek Inscriptions* ii. *CR* n.s. 25 (1975), 326–7.

H. Engelmann and R. Merkelbach, *Die Inschriften von Erythrai und Klazomenai* i. *CR* n.s. 25 (1975), 299–300.

R. Drews, *The Greek Accounts of Eastern History. CR* n.s. 25 (1975), 322.

J. Coupry, *Inscriptions de Délos* 89–104.33. *Gnomon* 47 (1975), 717–19.

1976

'Expansion of a Prytany-Decree', *ZPE* 20 (1976), 300.

* 'The Treaties with Leontini and Rhegion', *ZPE* 22 (1976), 223–5 = this volume, ch. 17.

REVIEWS

* J. S. Traill, *The Political Organization of Attica. AJA* 80 (1976), 311–12 = this volume, ch. 11.

N. Brockmeier and E. F. Schultheiss, *Studienbibiographie: Alte Geschichte. CR* n.s. 26 (1976), 285.

R. A. De Laix, *Probouleusis at Athens. CR* n.s. 26 (1976), 287.

K. E. Clinton, *The Sacred Officials of the Eleusinian Mysteries. JHS* 96 (1976), 213.

M. Stern, *Greek and Latin Authors on Jews and Judaism* i. *JTS* n.s. 27 (1976), 169–70.

J. D. Mikalson, *The Sacred and Civil Calendar of the Athenian Year. TLS* (30 April 1976), 533.

1977

Sparta and Persia, Leiden: Brill (Lectures in Memory of D. W. Bradeen. Cincinnati Classical Studies n.s. 1).

REVIEWS

H. W. Parke, *Festivals of the Athenians. TLS* (8 April 1977), 436.

J. Pouilloux, *Fouilles de Delphes*, III.iv. 351–576. *AJA* 81 (1977), 399.

O. Aurenche, *Les Groupes d'Alcibiade, de Léogoras et de Teucros. CR* n.s. 27 (1977), 74–5.

B. D. Meritt and J. S. Traill, *The Athenian Agora*, xv. *CR* n.s. 27 (1977), 93–4.

J. K. Anderson, *Xenophon. CR* n.s. 27 (1977), 107.

D. J. Mosley, *Envoys and Diplomacy in Ancient Greece*; F. E. Adcock and D. J. Mosley, *Diplomacy in Ancient Greece. CR* n.s. 27 (1977), 134–5.

Graffiti in the Athenian Agora. CR n.s. 27 (1977), 144.

T. B. Mitford and I. K. Nikolaou, *Salamis*, vi. *CR* n.s. 27 (1977), 145.

M. T. Manni Piraino, *Iscrizioni greche lapidarie del Museo di Palermo. CR* n.s. 27 (1977), 145.

Akten des VI. internationalen Kongresses für griechische und lateinische Epigraphik, München, 1972. CR n.s. 27 (1977), 145–6.

J. D. Mikalson, *The Sacred and Civil Calendar of the Athenian Year. CR* n.s. 27 (1977), 215–16.

M. Avi-Yonah, *The Jews of Palestine. CR* n.s. 27 (1977), 233–4.

W. Schuller, *Die Herrschaft der Athener im Ersten Attischen Seebund. CR* n.s. 27 (1977), 299–300.

E. Bayer and J. Heideking, *Die Chronologie des perikleischen Zeitalters. CR* n.s. 27 (1977), 300.

M. G. Bertinelli Angeli and M. Giacchero, *Atene e Sparta nella storiografia trogiana. CR* n.s. 27 (1977), 301.

H. A. Harris, *Greek Athletics and the Jews. JTS* n.s. 28 (1977), 555.
M. I. Finley, *The Use and Abuse of History. Eng. Hist. Rev.* 92 (1977), 354–5.

1978

'The Seleucid Inscription' in D. Stronach, *Pasargadae*, 160–1.

REVIEWS

B. Jordan, *The Athenian Navy in the Classical Period. CP* 73 (1978), 70–2.
J. A. Goldstein, *I Maccabees. CR* n.s. 28 (1978), 180.
E. W. Bodnar and C. Mitchell, *Cyriacus of Ancona's Journeys in the Propontis and the Northern Aegean, 1444–1445. CR* n.s. 28 (1978), 196.
E. M. Smallwood, *The Jews under Roman Rule. CR* n.s. 28 (1978), 319–20.
G. Pfohl (ed.), *Das Studium der griechischen Epigraphik. CR* n.s. 28 (1978), 381.
P. Ghiron-Bistagne, *Recherches sur les acteurs dans la Grèce antique. JHS* 98 (1978), 184–5.

1979

'An Inventory in the Agora', *ZPE* 36 (1979), 131–8.
Additional Note to C. W. Fornara, 'On the Chronology of the Samian War', *JHS* 99 (1979), 18–19.
(with R. S. Stroud) 'Athens honors King Euagoras of Salamis', *Hesperia* 48 (1979), 180–93.
(with D. W. Bradeen) 'Notes on Athenian Casualty Lists', *ZPE* 34 (1979), 240–6.

REVIEWS

M. L. Lang, *The Athenian Agora* xxi. *CR* n.s. 29 (1979), 125–6.
A. S. Henry. *The Prescripts of Athenian Decrees. CR* n.s. 29 (1979), 187.
M. B. Walbank, *Athenian Proxenies of the Fifth Century B.C. Phoenix* 33 (1979), 267–9.

1980

* 'Datis the Mede', *JHS* 100 (1980), 194–5 = this volume, ch. 33.
* 'Athena's Robe', *Scripta Classica Israelica* 5 (1979/80), 28–9 = this volume, ch. 16.
'The New Athenian Expense-Account', *ZPE* 37 (1980), 106–8.

REVIEWS

T. L. Shear, *Kallias of Sphettos and the Revolt of Athens in 286 B.C. JHS* 100 (1980), 257–8.
G. Nachtergael, *Les Galates en Grèce et les Sotéria de Delphes. JHS* 100 (1980), 258.
E. Ruschenbusch, *Untersuchungen zu Staat und Politik in Griechenland vom 7. – 4. Jh. v. Chr. CR* n.s. 30 (1980), 77–8.
R. E. Wycherley, *The Stones of Athens. CR* n.s. 30 (1980), 163–4.
F. Ferlauto, *Il testo di Tucidide e la traduzione latina di Lorenzo Valla. CR* n.s. 30 (1980), 276–8.

E. Schürer, rev. and ed. G. Vermes, F. Millar and M. Black, *The History of the Jewish People in the Age of Jesus Christ* ii. *JTS* n.s. 31 (1980), 583–4.

1981

Inscriptiones Graecae, I (editio tertia), fasc. i. Berlin: De Gruyter.

* 'The Origins of the First Peloponnesian War', *Classical Contributions . . . M. F. McGregor*, 71–8 = this volume, ch. 3.

REVIEWS

J. Seibert, *Die politischen Flüchtlinge und Verbannten in der griechischen Geschichte. CR* n.s. 31 (1981), 132–3.

E. J. Bickerman, *Chronology of the Ancient World* 2nd edn. *CR* n.s. 31 (1981), 309.

1982

* 'On the New Text of Teos', *ZPE* 47 (1982), 71–2 = this volume, ch. 2.

REVIEW

V. D. Gabrielsen, *Remuneration of State Officials in Fourth-Century B.C. Athens. JHS* 102 (1982), 269.

1983

'Themistocles' Mother', *Historia* 32 (1983), 245.

REVIEWS

T. J. Quinn, *Athens and Samos, Lesbos and Chios: 478–404 B.C. CR* n.s. 33 (1983), 146.

A. H. Sommerstein (ed.), *Aristophanes: Knights. CR* n.s. 33 (1983), 175–7.

* P. Siewert, *Die Trittyen Attikas und die Heeresreform des Kleisthenes. Gnomon* 55 (1983), 431–6 = this volume, ch. 12.

M. Stern, *Greek and Latin Authors on Jews and Judaism* ii. *JTS* n.s. 34 (1983), 393.

1984

* 'Democratic Institutions and their Diffusion', Πρακτικὰ τοῦ Η' Διεθνοῦς Συνεδρίου Ἑλληνικῆς καὶ Λατινικῆς Ἐπιγραφικῆς, *1982*, i.55–61 = this volume, ch. 8.

'Postscript, 1984', A. R. Burn, *Persia and the Greeks*, reissued, 587–609.

'Further Notes on Page, *Further Greek Epigrams*', *JHS* 104 (1984), 179–80.

'A Lost Notebook', *ZPE* 54 (1984), 118.

REVIEWS

V. L. S. Abel, *Prokrisis. CR* n.s. 34 (1984), 344–5.

A. S. Henry, *Honours and Privileges in Athenian Decrees. CR* n.s. 34 (1984), 357.

P. Krentz, *The Thirty at Athens. Phoenix* 38 (1984), 293–4.

T. Rajak, *Josephus: The Historian and his Society. JTS* n.s. 35 (1984), 501–2.

1985

* 'Persians in Herodotus', *The Greek Historians . . . A. E. Raubitschek*, 101–17 = this volume, ch. 34.

'The Archonship of Lysiades', *ZPE* 58 (1985), 271–4.

'A New Athenian Decree', *ZPE* 60 (1985), 108.

1986

'Violent Death in the Athenian Empire', *ZPE* 64 (1986), 184.

* 'Temple Inventories in Ancient Greece', M. Vickers (ed.), *Pots and Pans*, 71–81 = this volume, ch. 7.

REVIEWS

K. H. Rengstorf (ed.), *A Complete Concordance to Flavius Josephus* iii. *JTS* n.s. 37 (1986), 301–2.

M. Stern, *Greek and Latin Authors on Jews and Judaism* iii. *JTS* n.s. 37 (1986), 302.

1987

* 'The Athenian Coinage Decree', I. Carradice (ed.), *Coinage and Administration in the Athenian and Persian Empires*, 53–63 = this volume, ch. 15.

* 'The King's Dinner', H. Sancisi-Weerdenburg and A. Kuhrt (eds.), *Achaemenid History*, ii.79–87 = this volume, ch. 32.

'Bowie on Elegy: A Footnote', *JHS* 107 (1987), 188.

REVIEW

D. M. MacDowell, *Spartan Law*. *CR* n.s. 37 (1987), 231–2.

1988

(with J. Gould) A. W. Pickard-Cambridge, *The Dramatic Festivals of Athens* 2nd edn, reissued with Supplement and corrections, Oxford University Press.

(with R. Meiggs) *A Selection of Greek Historical Inscriptions to the End of the Fifth Century B.C.* revised edn (with Addenda and Concordance), Oxford University Press (See also under 1989.)

J. Boardman, N. G. L. Hammond, D. M. Lewis and M. Ostwald (eds.), *Cambridge Ancient History* iv, 2nd edn.

Includes chapter by D. M. L.: 'The Tyranny of the Pisistratidae', pp. 287–302.

'The Last Inventories of the Treasures of Athena', *Comptes et inventaires dans la cité grecque . . . en l'honneur de J. Tréheux*, 297–308.

'Lilian Hamilton Jeffery', *PBA* 73 (1987), 505–16.

'The Archon Charikles', *hόρος* 6 (1988), 19–20.

REVIEWS

'Inscriptions from Asia Minor', *CR* n.s. 38 (1988), 124–5.

E. Schürer, rev. and ed. G. Vermes, F. Millar and M. Goodman, *The History of the Jewish People in the Age of Jesus Christ* iii. *JTS* n.s. 39 (1988), 190–2.

1989

(with R. Meiggs) *A Selection of Greek Historical Inscriptions to the End of the Fifth Century B.C.* revised edn (with Addenda and Concordance), paperback edn, Oxford University Press.

'Professor Benjamin Meritt', *The Independent* (14 July 1989).

(with O. Palagia) 'The Ephebes of Erechtheis 333/2 BC and their Dedication', *BSA* 84 (1989), 333–44.

'Momigliano, Arnaldo Dante' and 'Tcherikover, Victor' in G. Abramson (ed.), *The Blackwell Companion to Jewish Culture* 527 and 752–3.

* 'Persian Gold and Greek International Relations', *REA* 91 (1989), 227–34 = this volume, ch. 36.

REVIEWS

M. Ostwald, *From Popular Sovereignty to the Sovereignty of Law*. *CR* n.s. 39 (1989), 279–81.

G. Petzl, *Die Inschriften von Smyrna* II.i. *CR* n.s. 39 (1989), 350–1.

1990

'The Political Background of Democritus', *Owls to Athens . . . Sir Kenneth Dover* 151–4.

* 'Public Property in the City', O. Murray and S. Price (eds.), *The Greek City from Homer to Alexander* 245–63 = this volume, ch. 9.

'The Synedrion of the Boeotian Alliance', A. Schachter (ed.), *Essays in the Topography, History and Culture of Boiotia* (*Teiresias* Supp. 3), 71–3.

* 'The Persepolis Fortification Texts', H. Sancisi-Weerdenburg and A. Kurht (eds.), *Achaemenid History* IV.1–6 = this volume, ch. 31.

'Brissonius: De Regio Persarum Principatu Libri Tres (1590)', H. Sancisi-Weerdenburg and J. W. Drijvers (eds.), *Achaemenid History* V.67–78.

'Professor Antony Andrewes', *The Independent* (15 June 1990).

REVIEW

R. W. Wallace, *The Areopagos Council, to 307 B.C.* *CR* n.s. 40 (1990), 356–8.

1991

REVIEWS

L. H. Jeffery, *The Local Scripts of Archaic Greece* 2nd edn revised by A. W. Johnston. *CR* n.s. 41 (1991), 265–6.

G. Petzl, *Die Inschriften von Smyrna* II.ii. *CR* n.s. 41 (1991), 266.

1992

The Jews of Oxford, Oxford Jewish Congregation.

D. M. Lewis, J. Boardman, J. K. Davies and M. Ostwald (eds.), *Cambridge Ancient History*, V, 2nd edn.

Includes chapters by D. M. L.: 'Sources, Chronology, Method', pp. 1–14; 'Mainland Greece to 449', pp. 96–120; 'The Thirty Years' Peace', pp. 121–46; 'The Archidamian War', pp. 370–432; 'Chronological Notes', pp. 499–503.

REVIEWS

H. W. Pleket and R. S. Stroud (eds.), *Supplementum Epigraphicum Graecum* XXXVII (1987). *CR* n.s. 42 (1992), 482.

M. L. Lang, *The Athenian Agora* XXV. *JHS* 112 (1992), 220–1.

1993

'Oligarchic Thinking in the Late Fifth Century', *Nomodeiktes . . . Martin Ostwald* 207–11.

* 'The Epigraphical Evidence for the End of the Thirty', M. Piérart (ed.), *Aristote et Athènes* 223–9 = this volume, ch. 24.

'Antony Andrewes, 1910–1990', *PBA* 80 (1993), 208–16.

* 'Megakles and Eretria', *ZPE* 96 (1993), 51–2 = this volume, ch. 14.

REVIEWS

H. W. Pleket and R. S. Stroud (eds.), *Supplementum Epigraphicum Graecum* xxxviii (1988). *CR* n.s. 43 (1993), 208.

J. Bingen, *Pages d'épigraphie grecque: Attique-Égypte (1952–1982). CR* n.s. 43 (1993), 208–9.

S. V. Tracy, *Attic Letter-Cutters of 229 to 86 B.C. JHS* 113 (1993), 215–16.

H. Duchêne, *La Stèle du port. CR* n.s. 43 (1993), 402–3.

J.-M. Bertrand, *Inscriptions historiques grecques. CR* n.s. 43 (1993), 460.

I. Worthington (ed.), *Acta of the University of New England International Seminar on Greek and Latin Epigraphy. CR* n.s. 43 (1993), 460–1.

1994

(with L. H. Jeffery and the assistance of E. Erxleben) *Inscriptiones Graecae* I (editio tertia), fasc. ii. Berlin: De Gruyter.

'The Persepolis Tablets: Speech, Seal and Script.' A. K. Bowman and G. Woolf (eds.), *Literacy and Power in the Ancient World* 17–32 with 218–20.

(untitled), B. Harrison (ed.), *Corpuscles* 200–5.

D. M. Lewis, J. Boardman, S. Hornblower and M. Ostwald (eds.), *Cambridge Ancient History*, vi, 2nd edn.

Includes chapters by D. M. L.: 'Sparta as Victor', pp. 24–44; 'Sicily, 413–368 BC', pp. 120–55.

'The Athenian Tribute Quota Lists, 453–450 BC', *BSA* 89, 1994, 285–301.

REVIEW

G. V. Lalonde, M. K. Langdon, M. B. Walbank, *The Athenian Agora* xix. *Phoenix* 48 (1994), 274–6.

1997

Selected Papers in Greek and Near Eastern History, Cambridge University Press = this volume.

Includes papers published here for the first time: 'The Athens Peace of 371', ch. 5; 'Aristophanes and Politics', ch. 21; 'The Financial Offices of Eubulus and Lycurgus', ch. 25; 'The Dating of Demosthenes' Speeches', ch. 26.

P. J. Rhodes with D. M. Lewis, *The Decrees of the Greek States*, Oxford University Press.

FORTHCOMING

'The Tomb of Theodoros', *hóros*.

'κατάλογοι θανόντων ἐν πολέμωι.' Ἀρχαιολογικὴ Ἐφημερίς.

Not published but deposited in the libraries of the Ashmolean Museum, Oxford; Christ Church, Oxford; Wolfson College, Oxford; Faculty of Classics, Cambridge; Hebrew University, Jerusalem; McGill University, Montreal; (obelised items only) Institute of Classical Studies, London:

† Some Athenian Attitudes.

† Thucydides' Use of Herodotus.

† Achaemenid History: Bibliography.

Ezra-Nehemiah and the Persian Empire.

Untitled paper on the Persepolis Fortification Tablets.

† Untitled paper on Herodotus and the Persian Empire.

Variants in the Behistun Inscription.

Material assembled by D.M.L. is to be used in articles by:

D. Harris-Cline: on Athenian temple inventories.

C. J. Tuplin: 'The Seasonal Relocation of Achaemenid Kings: A Report on Old and New Evidence.'

I INDEX OF TEXTS TREATED IN DETAIL

For the inscriptions included this index offers cross-references between the principal editions cited in the text, but not a complete concordance. Some more general references to literary texts and their authors are included in the General Index.

Inscriptions

Agora I 3771: *see SEG* xxi 570–9
5477: *see SEG* xviii 13
AM 67 (1942), 17–20: *see SEG* xxi 570–9
'Αρχ. 'Εφ. 1948/9, 146ff.: *see IG* i³ 1330
1952, 194ff.: 380–2
ATL, A9: *see IG* i³ 71
D2: *see IG* i³ 52.B
D14: *see IG* i³ 1453
BCH 78 (1954), 316–22
Behistun Inscription: 346, 349
Chiron 11 (1981), 1–30: *see SEG* xxxi 985
Corinth, viii.2 54: 200–1
De Franciscis, *Stato e società in Locri Epizefiri*: 32–9
Grayson, *Assyrian Royal Inscriptions* ii, §682: 332–41
Hesperia, 6 (1937), 456–7 no. 6: 226–7
9 (1940), 330–2: *see SEG* xxi 570–9
10 (1941), 42 no. 10: *see SEG* xix 119
28 (1959), 239–47: *see SEG* xviii 13
29 (1960), 2–4 no. 3: *see SEG* xix 119
IG i² 45: *see IG* i³ 46
58: *see IG* i³ 63
71: *see IG* i³ 89
114: *see IG* i³ 105
128: *see IG* i³ 130
189: *see IG* i³ 245
i³ 14: 56
52: 137–8
53: 133–5
54: 133–5
63: 144
71: 143–4
89: 144–5
93: 137–8
105: 203–4
130: 150–7
245: 137
247: 137
380: 208–10
421–30: 158–72
1330: 187–9
1453: 116–30, 139–42, 149
ii² 43: 23–4, 31, 67, 147–8
215: 214
334: 252–62
336: 226–7
404: 27–8
1128: 23
1370: 206–8
1371: 206–8
1384: 206–8
1456: 196–7
1471: 264
1493: 226–7
1494: 226–7
1496: 226
1497: 226–7
1503: 206–8
1580: *see SEG* xxi 570–9
1594–1603: *see SEG* xxi 570–9
1622: 218–21
2776: 290
3453: 192–4
3461: 198
4126: 200–1
xii.5 594: 22–8
Inschriften von Erythrai und Klazomenai, 2: 56–7
Iraq 14 (1952), 22–44: *see* Grayson
Klio 52 (1970), 163–73: 54
Michel, 533: 53
ML 40: *see IG* i³ 14
45: *see IG* i³ 1453

49: *see IG* i³ 46
58: *see IG* i³ 52
63: *see IG* i³ 53
64: *see IG* i³ 54
69: *see IG* i³ 71
78: *see IG* i³ 93
Pritchard, *Ancient Near Eastern Texts*³ 560: *see* Grayson
SEG x 86: *see IG* i³ 89
xii 80: *see IG* i³ 1330
xiv 530: 22–8
xviii 13: 252–62
15: 66
xix 119: 227–8
xxi 570–9: 263–93
xxiii 22: *see IG* i³ 247
81: *see IG* ii² 1370 + 1371 + 1384 + 1503
xxxi 985: 7–8
*SIG*³ 279: 67–8
Tod 123: *see IG* ii² 43
141: *see IG* xii.5 594
162: *see IG* ii² 1128

Papyrus/leather

Cowley, *Aramaic Papyri*, nos 27, 30, 31: 362–8
Driver, *Aramaic Documents*, v, vii, viii: 362–8

Other

Aeschines, iii.25: 215–21
[Aristotle], *Ath. Pol.* 29.4: 145–7
43.1: 212–5
Econ. ii.1346b13f.: 266
Rhet. ad Alex. ii.1425b19f.: 266
Athenaeus, iv.145–6: 333–4
Heraclides of Cumae, *FGrH* 689f2: *see* Athenaeus
Hyperides, *Dem.* 28ff.: 222–4
Polyaenus, iv.3.32: 332–41
Strattis, fr. 30 Kock = Edmonds = Kassel and Austin: 131–2
Thucydides, ii.24: 138–9
viii.15.1: 138–9
59: 362–8
67.2: 145–7
87: 362–8
108.1: 363
Xenophon, *Hellenica* vi.5.1–vii.1.2: 29–31

2 GENERAL INDEX

The spelling of Greek words and names is not uniform between different chapters of this book, but it is hoped that readers will not be seriously inconvenienced. Persons (other than modern scholars) and institutions not otherwise identified are Athenian.

Abdera. Quorum at, 7–8
Aegina. In First Peloponnesian War, 9–21
Aesillas. Quaestor of Macedonia, overstriking Athenian coins, 316–18, 324
Alcibiades. Profanation of Mysteries, 158–9, 164–5, 167–8, 172
Alcmeonids. Properties, 78–9; in Cleisthenes' tribal reform, 89–90, 95–8
Alexandra. Priestess of Athena Polias, 199
Alketes. Mint-magistrates, 309
Amendments to decrees, 58–9
Amphias. Mint-magistrate, 310
Amphikrates. Mint-magistrate, 310–11
Amphikratides. Mint-magistrate, 310–11
Andrejev, V. On *rationes centesimarum*, 263–76
Antigrapheus. In 4th century, 216–17
Apagoge. 141–2, 146
Apellikon. Mint-magistrates, 297, 307
Apodektai. In 4th century, 217
Apollo Delios. Cult at Phaleron, 150–7
Aquillius Florus Turcianus Gallus, L., Roman magistrate. Dating, 200–1
Argos. In First Peloponnesian War, 9–21; public entries in competitions, 72
Aristion. Mint-magistrate, 294–5, 297–8, 301, 323
Ariston. In family of Timostratos, 312
Aristonikos. Proposer of law on Panathenaea, 252–62
Aristonous.Mint-magistrate, 307
Aristophanes. And politics, 173–86; *Acharnians*, 175–6, 183–4; *Birds*, 116; *Frogs*, 181–2; *Knights*, 184–5; *Lysistrata*, 181, 187–95
Asklapon. Mint-magistrate, 307
Asklepiades. Mint-magistrate, 312–13
Assembly, *see Ekklesia*
Athena Nike. Myrrhine priestess of, 187–9
Athena Polias. Lysimache priestess of, 186–95; priestesses of, 196–202; *peplos* of, 131–2. *See also* Panathenaea
Athenion. Priestess of Athena Polias, 202
Athenobios. Mint-magistrate, 308
Athens. Boeckh on *Staatshaushaltung*, 1–6; in First Peloponnesian War, 9–21; treatment of Keos, 22–8; common peace of 371/0, 29–31; temple inventories, 40–50; influence on institutions of other states, 51–9; public property, 60–76; Cleisthenic tribes, 77–109; ostracism, 110–15; coinage decree, 116–30; *peplos*, 131–2; treaties with Leontini and Rhegion, 133–5; entrenchment-clauses in decrees, 136–49; cult of Apollo Delios, 150–7; confiscations after 415, 158–72; Aristophanes and politics, 173–86; priestesses of Athena, 187–202; republication of laws, 203–4; sacred treasurers of 404/3 and 403/2, 205–11; Eubulus and Lycurgus, 212–29; Demosthenes, 230–51; law on Lesser Panathenaea, 252–62; *rationes centesimarum*, 263–93; New Style coinage, 294–324; Persian gold, 369–79
Attic Stelai, 158–72

Boeckh, A. *Staatshauhaltung*, 1–6, 263
Boule. With *demos* in decree prescripts (outside Athens), 54, 57–8; in Erythrae, 56; oath of and laws about (in Athens), 203–4
Boutadai. In Cleisthenes' system, 78, 82, 85–6

Brauron. Home of Peisistratus, 79; in Cleisthenes' system, 83

Caecilius of Kale Akte. On Demosthenes, 236–7
Caesar, Roman magistrate. On Macedonian coins, 317–18
Callimachus of Cyrene. On the Attic orators, 231, 232, 234, 238, 240, 243
Cawkwell, G. L. On dating of Demosthenes' speeches, 230
Chalcis. 2nd-century coinage, 314–16, 324
Chrysis. Priestess of Athena Polias, 199
Claudius Anaxilas, T., Roman magistrate. Dating, 200–1
Claudius Novius, T. Dating, 199–200
Cleisthenes. Tribes, trittyes and demes, 77–109, cf. 53–4
Cleonae. In First Peloponnesian War, 14–16, 20
Coinage. In Delian League, 116–30, 139–42, 149; New Style, 294–324; Persian, used to influence Greece, 369–79
Confiscation of property, 61–3, 158–72
Corinth. In First Peloponnesian War, 9–21; in common peace of 371/0, 29–31; confiscation of property in, 62
Cos. Democracy in, 57–8; fragment of Athenian coinage decree, 117–18, 120, 122–3, 125
Ctesias, writer on Persia. On Persian dinners, 335; compared with Herodotus, 346, 349–50
Cyrus, Persian prince. Dealings with Greeks, 374–6

Datis, Persian commander. In Ionian Revolt, 342–4, 356
Decrees. Formulation (in Athens and elsewhere), 51–9; entrenchment-clauses (in Athens), 136–49
Deinon of Colophon. On Persian dinners, 334–5
Delian League. Keos in, 24; Athenian influence on institutions in, 52–7; Athenian coinage decree, 116–30; Athenian property in, 164, 166–7
Delos. Temple inventories, 47–8; confiscated property, 63
Delphi. Quorum at, 7; temple inventories, 49; confiscated property, 63
Demes. In Locri, 34–7; in Cleisthenes' reforms, 77–109; in *rationes centesimarum*, 265, 268–9, 277–89
Demetrios. Sculptor, of deme Alopeke, 191–4
Democracy. Athenian influence in other states, 52–7; extent of democracy outside Athens, 57–9
Demos. Meaning, 74–6. *See also Ekklesia*
Demosios. Meaning with reference to property, 60–74
Demosthenes. *Epi to theorikon*, 223–5; dating of speeches, 230–51, 262; receiving Persian gold, 377
Demoteles. Meaning, 61
Diakrioi. Location, 79–80
Dioikesei, ho epi tei, 215, 227–8
Didymus of Alexandria. On Demosthenes, 244–5
Dionysios. Mint-magistrate, 308
Dionysius of Halicarnassus. *Ad Ammaeum*, 230–51, cf. 262; *Deinarchos*, 231–5
Diophantos. On public slaves, 71
Dover, K. J. On Callimachus, 231, 234; on dating of speeches, 240–2; on speech-writers, 246

Ekklesia. With *boule* in decree prescripts (outside Athens), 54, 57–8; payment (outside Athens), 58; enactment of Cleisthenes' reforms, 96; powers of *boule* and *ekklesia* (in Athens), 203–4
Electrum coinage. In Delian League, 128–9
Eleusis. Slaves employed at, 69–70; profanation of Mysteries, 158–72
Elis. In common peace of 371/0, 29–31
Endeixis. 141, 146–7
Epistratos. Mint-magistrates, 310–11
Eretria. And Keos, 25–6; decrees, 52–3, 58; *epimenioi*, 58; in Megakles *ostrakon*, 114–15; 2nd-century coinage, 314–16, 324
Erythrai. Athenian influence, 52–3, 56–7; democracy, 58
Eschatia. Marginal land, 291–3, cf. 273–4, 276, 277–89
Eteoboutadai. And deme of Boutadai, 82, cf. 78; priestesses of Athena, 190–202

Euagion. Mint-magistrate, 309
Eumareides. Mint-magistrate, 309–10
Eubulus. And Athenian finance, 212–21, cf. 66, 71

Flavia Phainarete. Priestess of Athena Polias, 201–2

Habryllis. Priestess of Athena Polias, 198–9
Halimous. In Cleisthenes' system, 89–90
Hegemon. Proposer of law on financial offices, 221, 224–5
Hekale. In Cleisthenes' system, 88–9
Heraclides of Cumae. On Persian dinners, 323–5
Hermae. Mutilation in 415, 158–72, cf. 62
Hermippos of Smyrna. On the Attic orators, 232, 236, 238–40, 243–4
Herodotus. Knowledge of Persian Empire, 345–61
Hikesios. Mint-magistrate, 312–13
Hipposthenis. Priestess of Athena Polias, 200–1
Horoi, 64–5, 92
Hyperakrioi. In 6th century, 79–80

Iasus. Assembly paid, 58
Inventories, 40–50, 206–11
Ioulis. Status within Keos, 22–8
Isagoras. Home, 81–2; in Cleisthenes' reform, 88–9; status when reforms enacted, 96

Jews. In Elephantine, 346; in Greece, 380–2; knowledge of Greek, 383–8
Josephus. On knowledge of Greek, 385–6
Junia Megiste. Priestess of Athena Polias, 199–200

Kall[i——]. Priestess of Athena Polias in 220/19, 198
Kallist[——]. Priestess of Athena Polias 1st century AD, 199
Karthaia. Status within Keos, 22–8
Keos. Federation and *poleis*, 22–8
Klearchos. Coinage decree(s), 116–30
Koresia. Status within Keos, 22–8
Kyzikos. Athenian influence, 53; electrum staters, 128–9

Lamios. Mint-magistrate, 307
Land-holding, 60–76, 158–72, 256–60, 263–93
Leontini. Alliance with Athens, 133–5
Libanius of Antioch. On Demosthenes, 241–2, 249–51
Lindos. Athenian influence, 54
Locri, Epizephyrian. Bronze tablets from, 32–9
Lycurgus (6th century). Of Boutadai, 78
Lycurgus (4th century). And Athenian finance, 221–9, cf. 42, 266
Lysimache (5th century) Priestess of Athena Polias, 189–95
Lysimache (3rd century). Priestess of Athena Polias, 191, 197–8
Lysistrata. Aristophanes' representation of Lysimache, 189–95

Mantinea. In common peace of 371/0, 29–31
Marathon and Marathonian tetrapolis. And Peisistratus, 78–80; in Cleisthenes' system, 87–8, 93, 104, 106
Mattingly, H. B. Dating of 5th-century inscriptions, 116–30, 133–5, 149
Megakles. Ostracism, 110–12, 114–15
Megara. In First Peloponnesian War, 9–21
Megiste. Priestess of Athena Polias, 199. *See also* Junia Megiste
Menoites. Mint-magistrate, 307
Miletos. Athenian influence, 53–4, 58; democracy, 57–9; amendments to decrees, 59; rejects public flocks, 72 n. 32
Mines. Public property, 63
Mithradates V / VI of Pontus. As Athenian mint-magistrate, 294–5, 297–8, 301, 323
Myrrhine. Priestess of Athena Nike, 187–9
Mysteries, Eleusinian. Profanation of, 158–72

Ne[——]. Priestess of Athena Polias 1st century AD, 201
Nea. Territory leased in 4th century, 252–62, cf. 66
Neorion epimeletai, 217–20
Niketes. Mint-magistrate, 308
Nikonomos. Mint-magistrate, 307
Nomoi. Used 4th century to regulate finance, 255, 260–2

Oinophilos. Mint-magistrate, 310
Oropos. Recovered by Athens after Chaeronea, 66, 252 n. *, 254; Jew manumitted at Amphiareion 3rd century, 380–2
Ostracism. Kerameikos hoard, 110–13; Megakles, 114–15

Panathenaea. *Peplos*, 131–2; used in defining terms of office, 212–15; law on financing of Lesser Panathenaea, 252–62
Paralioi. In 6th century, 78–9
Paros. Amendments to decrees, 58–9
Patron. Mint-magistrate, 311
Paulleina Scribonia. Priestess of Athena Polias, 202
Pedieis. In 6th century, 78
Peisistratus, Peisistratids. Confiscations of property, 62; home of, 77–80; in Cleisthenes' system, 93
Peloponnesian War, First, 9–21
Peloponnesian War. Athenian use of temple treasuries, 41–2; Persian navy in 411, 362–8; Persian gold, 373–6
Penteteris. Priestess of Athena Polias, 198
Peplos. Of Athena, 131–2
Pericles. In First Peloponnesian War, 20; attitude to temple treasuries, 41–2
Persepolis. Fortification Texts, 325–61
Persian Empire. Persepolis Fortification Texts, 325–61; royal dinners, 332–41; Datis, 342–4; Herodotus' knowledge of, 345–61; navy in 411, 362–8; gift-giving, 369–79
Phaleron. Cult of Apollo Delios at, 150–7
Pharnabazos. Persian satrap 5th–4th centuries, 374–6
Phanostrate. Priestess of Athena Polias, 196–7
Philaidai. Deme in Cleisthenes' system, 83, 104
Philaids. Home of, 80–1; in Cleisthenes' system, 83
Philippe. Priestess of Athena Polias, 199
Philotera. Priestess of Athena Polias, 199
Piraeus. Public property in, 64–5; coin-tester at, 71
Poiessa. Status within Keos, 22–8
Polemon. Mint-magistrate, 311
Poletai. Leasing public land, 66; selling confiscated property, 158–72
Poses. Mint-magistrate, 312
Probalinthos. In Cleisthenes' system, 87–8, 106
Prytaneis. In states other than Athens, 53–6

Rhegion. Alliance with Athens, 133–5
Rhodes. Athenian influence in Lindos, 54; assembly paid, 58

Sabeiniane. Priestess of Athena Polias, 202
Sealey, R. On 6th-century 'parties', 78; on Isagoras, 81; on dating of Demosthenes' speeches, 230–48
Sevenster, J. N. On Jews' knowledge of Greek, 383–8
Siewert, P. On Cleisthenes' tribal system, 102–9
Skotoussa. Public property in, 67
Slaves, public, 67–71
Sparta. In First Peloponnesian War, 9–21; in common peace of 371/0, 29–31; hoping for Persian navy in 411, 362–7; receiving Persian gold, 373–7
Stoichedon. Style in inscriptions, 52–3
Stratiotic fund. Treasurers of, 214–15
Stratokleia (1st century BC). Priestess of Athena Polias, 199
Stratokleia (1st century AD). Priestess of Athena Polias, 201
Sura, Roman magistrate. On Macedonian coins, 317–18
Swoboda, H. On Greek decrees, 51–9

Tamiai of Athena and Other Gods. In 404/3 and 403/2, 206–11. *See also* Inventories
Taxation. Of produce, 258–9; of special land sales, 263–93
Tegea. In common peace of 371/0, 29–31
Teos. Quorum at, 7–8
Themistocles. Mint-magistrates, 311
Theodote. Priestess of Athena Polias, 198
Theophrastos. Mint-magistrates, 311
Theoric fund. Treasurers of, 212–21, 223–5
Thirty. Confiscation of their property, 62, 160–1, 172; sacred treasurers under Thirty and afterwards, 205–11
Thompson, M. On New Style coinage, 294–324

Timostratos. Mint-magistrate, 312
Tissaphernes. Persian satrap 5th–4th centuries, 362–8, 374–6
Traill, J. S. On Cleisthenes' tribal system, 99–101, cf. 102–3, 106, 108–9
Treasuries, sacred, 40–50, 206–11
Tribes. Cleisthenic, 77–109; in *rationes centesimarum*, 265, 267–70, 277–89; outside Athens, in Locri, 34–7; in Miletos and Kyzikos, 53–4, in Erythrai, 56
Trittyes. Of Cleisthenic tribes, 77–109, esp. 83–7, 92–4, 102–9; of *prytaneis*, 103, 105–6

Xenokles. *Epi tei dioikesei*, 227–8
Xenophon. *Poroi*, 63, 65–6, 68

Zelea. Public property in, 67–8